FISCAL YEAR 1983 DEPARTMENT OF ENERGY BUDGET REVIEW

(Fossil Energy and Basic Research)

HEARINGS

BEFORE THE

SUBCOMMITTEE ON
ENERGY DEVELOPMENT AND APPLICATIONS

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

NINETY-SEVENTH CONGRESS

SECOND SESSION

MARCH 18, 23, 24, 25, 30, 1982

[No. 111]

VOLUME III

Printed for the use of the
Committee on Science and Technology

U.S. GOVERNMENT PRINTING OFFICE
94-648 O WASHINGTON : 1982

H701-82

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FISCAL YEAR 1983 DEPARTMENT OF ENERGY BUDGET REVIEW

(Fosssil Energy and Basic Research)

THURSDAY, MARCH 18, 1982

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
Washington, D.C.

The subcommittee met, pursuant to notice, at 2 p.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Present: Representatives Fuqua, Volkmer, Nelson, Gregg, Sensenbrenner, Skeen, Walker, Schneider, and Gore.

Mr. Fuqua. The subcommittee will be in order.

We begin our review of the fiscal year 1983 budget for fossil energy research and development programs. This current budget request by the Department of Energy represents a reduction of 87 percent from the 1981 funding level.

Such Federal funding reductions reflect the Department of Energy's view concerning the private sector's ability and desire to proceed with development of these technologies.

Secretary Edwards has testified that the fossil energy industry can pursue technology development on its own. There have been some serious questions about those assumptions.

In addition, this committee has expressed its concern with the imbalance between the nuclear and the non-nuclear budget requests.

In the past 8 years, the Federal support for energy research and development has brought together exceptionally talented scientists and engineers. Their work has produced innovative processes and new projects, some of which will soon be commercialized.

This manpower in our universities, national laboratories, and industry, which has been responsible for these advances, represents a national resource.

We share the administration's concern with the erosion of America's technological edge. We are, therefore, deeply distressed with proposed funding levels that will prematurely close out future technology options and disband our research teams.

It would be tragic and very detrimental at this time for this to occur when the country's energy security and industrial competitiveness are at risk. We must not forsake our future to chance.

In the next few days, these hearings will address both general policy issues and specific programs in fossil energy. We anticipate that a healthy and thoughtful discussion of the proper role of the Federal Government in energy research, development, and demonstration.

Also, since our friends in other countries look to us for leadership, we must give careful consideration to the international policy and national security implications of the Department of Energy proposals.

We want to welcome this morning, Mr. Jan Mares, the Assistant Secretary for Fossil Energy and his first appearance before the subcommittee.

We are happy to have you here and will be happy to hear your testimony.

Your written testimony will be inserted into the record.

STATEMENT OF JAN W. MARES, ASSISTANT SECRETARY FOR FOSSIL ENERGY, U.S. DEPARTMENT OF ENERGY, ACCOMPANIED BY DONALD BAUER, DEPUTY ASSISTANT SECRETARY FOR FOSSIL ENERGY, U.S. DEPARTMENT OF ENERGY; AND, CARL GUIDICE, ACTING DIRECTOR OF MANAGEMENT AND PLANNING, U.S. DEPARTMENT OF ENERGY

Mr. MARES. Thank you, Mr. Chairman.

As you have noted, we have submitted a detailed statement describing the individual components of our 1983 budget submission, which has been included in the record.

Although, the formal statement is lengthy—it is more than 30 pages—we recognize that the committee has many questions about our budget submission. We have attempted to address these questions in as much detail as possible.

We are also prepared to discuss the proposed changes in the 1982 budget which we believe are necessary to insure a smooth transition to 1983.

Rather than summarizing each of the line items included in our formal statement, Mr. Chairman, I would propose spending a few minutes describing the policy considerations that form the backdrop for this budget. After that, I will be pleased to respond to any questions that you or other members of the committee may have. If I am not able to respond fully to your questions, members of my staff are here who can provide additonal details. With me are Mr. Donald Bauer, my Deputy, and Mr. Carl Guidice, the Acting Director of Management and Planning. Also accompanying me are the Fossil Energy Office Directors.

In formulating this budget, there has been no fundamental departure from the view that this committee, and others, have expressed on numerous occasions; namely that fossil fuels will continue to be the dominant supplier of energy in the United States for decades to come. I have included a graph in the opening section of my statement which shows the mid-range projections of the Department of Energy's national energy policy plan. It graphically illustrates this point.

The key distinction between this budget and previous ones is that it recognizes there are better mechanisms than direct Federal funding to move fossil energy technologies into commercial use. We have devoted a portion of the opening section of our statement to describing such mechanisms: the decontrol of energy prices; regulatory and leasing reforms; tax incentives; and the role of the Synthetic Fuels Corporation.

Much of this has been made possible with the support of this committee. We believe that each will play a major role in assuring that fossil fuels are able to compete and thrive as the marketplace dictates.

REORIENTATION OF FOSSIL R. & D.

This, in turn, has resulted in a fundamental reconfiguration of the Federal fossil energy research and development program. As Secretary Edwards described in his opening testimony before you last month, the fossil energy program would focus on the early stages of research and development—on long-term, high-risk research that may have substantial future benefits but is beyond the time frame or scope of the private sector.

There are no mathematical formulas that define for all cases where public and private sector responsibilities may be divided. But, perhaps by referring to the typical development path of a new technology, as we have shown on page 2 of the formal statement, it may be easier to visualize what we mean by "high-risk, long-term" research and development.

As you can see by the chart, the Government's role is concentrated on basic science and applied research where the dominant questions are more technical in nature. These stages of development remain unlikely to attract sufficient private funding because of their generic or high-risk nature or the substantial time required before they return commercial benefits.

Beyond those stages, where market factors begin to play an increasingly important role in process development and commercialization, we believe industry should assume the initiative.

PROOF OF CONCEPT

In some technologies, it may be necessary for the Government to continue through the "proof of concept " stage before the risk of a new technology is reduced to the point that industry will continue development. This was the case for several coal gasifiers in our past R. & D. program. Once the technical feasibility of several processes was shown in small-scale units—a few tons per day in size—many firms began their own development and commercialization programs.

We have some proof of concept projects in this budget; the Grimethorpe pressurized fluidized bed unit and the Mobil M-gasoline pilot plant, for example. Although no new funding is requested, we have sufficient funds from past years to continue proof of concept work on advanced liquefaction techniques at the 6-ton per day Wilsonville test unit. We anticipate that each project will provide industry the data it needs to make future development and commercialization decisions.

For some technologies, the private sector may be able to move forward following laboratory and bench-scale research. The dry scrubber applied to high-sulfur coals, for example, began to attract commercial interest once its feasibility was shown using a laboratory-scale furnace. Similarly, preparation techniques for coal-oil mixtures were developed in laboratory experiments, then were moved by private firms directly into commercial applications.

It is difficult, therefore, to apply a rigid definition of the proper Federal role that holds true for all technologies. Yet, one general conclusion can be drawn in almost all areas, Government research to date has proven most effective when it concentrated on the early stages of development, on basic and applied research and, in some cases, on proof of concept.

The $107 million in budget authority requested for the fiscal year 1983 fossil energy program reflects the fact that research of this nature is inherently less expensive than constructing large-scale pilot and demonstration plants. It also recognizes that the need is even greater this year to restrain Federal spending, and in light of overriding economic concerns, not all promising energy concepts can be pursued.

PRIORITIES IN FOSSIL R. & D. PROGRAM

Nearly 50 percent of our budget request relates directly to developing new sources of petroleum or providing substitutes for conventional liquid fuels. This is our highest priority because liquids remain a primary source of national concern about energy. Technology base research into the liquefaction of coal, the retorting of oil shale, the recovery of additional oil from conventional reservoirs, and the development of technologies that allow coal to displace oil make up the bulk of this activity.

The remainder of our budget request is divided between gaseous fuel technologies and the direct utilization of coal. Included in the gas-related activity is the conversion of coal to a synthetic gas which could be used as a feed stock for liquid fuels or chemicals, or upgraded to pipeline quality, or used in a fuel cell or in a combined cycle power plant.

The direct utilization program includes technologies for using coal in a more economical, efficient, and environmentally acceptable manner. This involves developing new control systems for cleaning coal before combustion and removing pollutants form the postcombustion gases, as well as research into new combustion concepts.

TRANSFER OF ACTIVITIES

Each of these activities are currently planned to be transferred to the Commerce Department, if Congress agrees. We are proposing to shift one program—mining research and development—back to the Department of the Interior where it would be combined with work already underway to improve the safety and environmental impact of coal extraction.

DISCONTINUATION OF SOME ACTIVITIES

There are three activities we are proposing not to continue in 1983. One is the heat engines program, where we believe industry is well positioned, by virtue of activities it has underway already, to continue to develop many of the technology improvements—in higher temperature turbines, for example.

Another is magnetohydrodynamics, where the issue is not whether the technology is of marginal or greater benefit, but simply that it is a process that requires a large-scale, expensive proof of concept effort.

The third is the enhanced gas recovery program, where industry activity is also increasing and many of the Federal projects are now reaching logical conclusions.

DIFFICULT BUDGET CHOICES

Finally, Mr. Chairman, we are not reluctant to admit that this budget was forged from a series of hard choices, in which individual program objectives were balanced against overall national priorities.

In some cases, we chose to eliminate some programs entirely rather than disperse small amounts of funding across too broad a technological spectrum. The fact that a technology is not being supported does not mean that past work in it was not useful; instead, it means that when weighed against other priorities and when we considered the other economic incentives now in place, our judgment was that other technical areas were more appropriate recipients of Federal dollars.

Therefore, we ask this committee to judge the funding proposed for the fossil energy program in the context of a broader energy strategy involving less Government spending, less regulation and less taxation. We believe that, coupled with these other incentives, this budget represents a realistic approach to building a sound knowledge base from which the fossil energy industry can select the technological options it deems most appropriate.

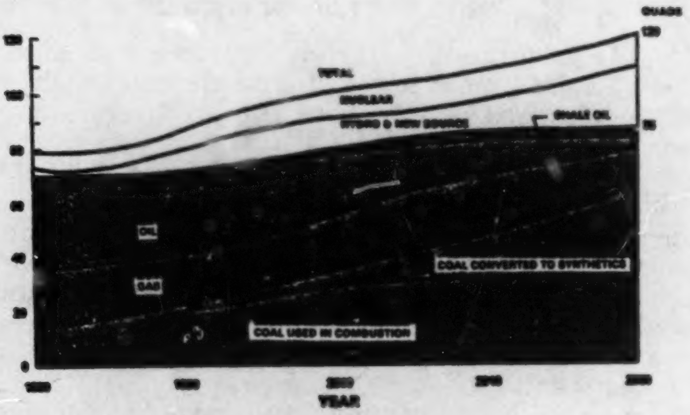
Thank you, Mr. Chairman. I will be pleased to answer your questions.

[The prepared statement of Mr. Mares follows:]

PREPARED STATEMENT OF JAN W. MARES, ASSISTANT SECRETARY FOR FOSSIL ENERGY

Mr. Chairman and Members of the Committee:

The portion of the Administration's proposed fiscal year 1983 budget which we will describe today recognizes that fossil fuels — coal, petroleum, and natural gas — are currently the dominant suppliers of energy in the U.S. and will continue to be for decades to come. Today, 90 percent of our energy is derived from these fuels, and forecasts of the nation's energy mix well into the next century (such as the projections of the National Energy Policy Plan shown below) show a continued national reliance on fossil energy, perhaps supplying up to 70 percent of our total energy needs in the year 2020.



NATIONAL ENERGY POLICY PLAN
PROJECTED U.S. ENERGY SOURCES

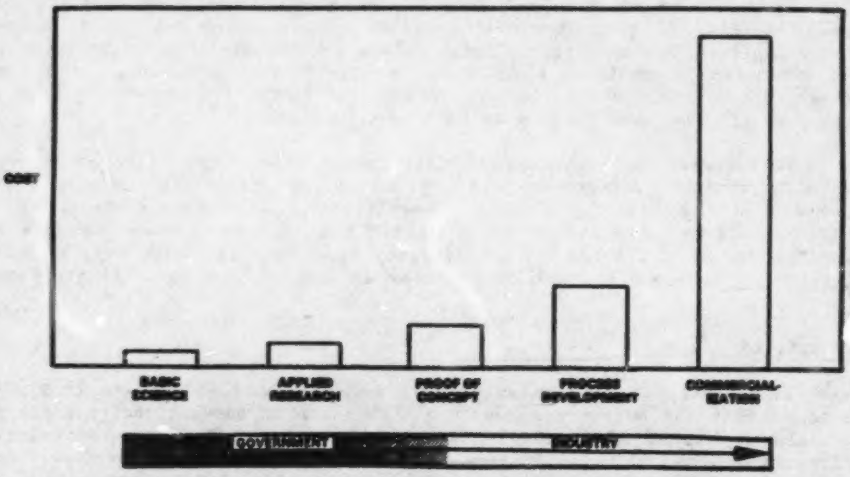
One benefit of this heavy reliance on fossil fuels is the strong technical capability of the industries that produce these fuels and that manufacture the hardware to utilize them. The policy on which this budget is based recognizes that these industries historically have propelled our nation's technological progress in energy. Given today's economic incentives, this sector is well positioned to continue and expand its role in developing and marketing new fossil energy technologies with minimum government help. Government has played an important role, most notably in such areas as coal-oil mixtures, atmospheric fluidized bed combustion, and new synthetic fuel processes, but the market continues to be the ultimate judge as well as the ultimate beneficiary of new energy technology.

Therefore, the proposed budget for fossil energy research and development in FY 1983 continues the process begun by the Administration last year to 1) reshape Federal research and development (R&D) programs to long-term, high-risk activities so they complement today's private sector initiatives, and 2) restore proper dimensions to Federal spending.

The redirection of Federal fossil energy R&D has reduced the FY 1983 funding requirement to $106.9 million, compared to the FY 1982 appropriation of $416.9 million. Within this budget, we have eliminated activities that overstep the proper sphere of Federal Government responsibilities, and we have better targeted and streamlined those generic and technology base R&D activities that remain appropriate for the Federal Government.

THE FOCUS ON "HIGH-RISK, LONG-TERM" CONCEPTS

A central premise of this budget is that the most effective use of direct Federal outlays is through a program focused on long-term, high-risk research that is beyond the time horizons of private companies. We recognize that the description "long-term, high-risk" cannot be defined precisely; however, to describe generally the boundaries of Federal involvement reflected by this term, it may be helpful to visualize the development milestones a new technology typically passes as it moves from laboratory to marketplace. These are displayed on the following chart:



"Long-term, high-risk" research focuses the Government's role on the early stages of this development path, that is, on basic science and applied research, and, in some cases, through "proof-of-concept." These stages remain unlikely to attract sufficient private funding because of their generic or high-risk nature or the substantial time required before they return commercial benefits. Yet, the payoffs of research in these areas, in terms of improved efficiency, economics, or environmental performance, may ultimately be substantial. An additional reason why industry is reluctant to undertake basic research and development is that it cannot exclude competitors from using the results. Therefore, a further justification for Federal involvement in basic research and development is that the information generated is for the public good.

Beyond these early development stages, Government is not well suited to make the market-related judgments that govern many of the decisions. Stable economic and regulatory policies can be more effective in moving new technologies through these final phases of development and into the marketplace than direct government funding.

The diversity of fossil energy technologies required sufficient flexibility in selecting candidates for funding to accomodate such factors as:

- differences among technologies;
- differences among technology developers and users;
- changing market factors and conditions;
- the size, complexity and economic uncertainties of projects being considered; and
- the resource affected.

These factors influence the point at which private firms are willing to risk R&D funds, and where the Government's role should end.

There is no mathematical formula for drawing the line dividing the responsibility of government and industry in developing new fossil energy technologies. For most technologies, there undoubtedly will be an overlap, as evidenced by projects which involve significant co-funding. In some cases, the point at which industry assumes the initiative may not be finally known until government funding is no longer available.

The process we undertook in formulating this budget, therefore, involved balancing projects against a given set of priorities and within the context of the need for fiscal restraint. In some cases, we chose to eliminate some programs entirely rather than disperse small amounts of funding across too broad a technological spectrum. In this way, we have retained a reasonable level of activity in the highest priority programs.

Priorities

In general, we have given highest priority to building the knowledge base so that the Nation can reduce its reliance on conventional sources of petroleum. In FY 1983, nearly 50 percent of our budget request relates directly to developing new sources of petroleum or providing substitutes for conventional liquid fuels. Research into the liquefaction of coal, the retorting of oil shale, the recovery of additional oil from conventional reservoirs, and the development of technologies that allow coal to displace oil make up the bulk of this funding.

The remainder of our budget request is divided between gaseous fuels and the direct utilization of coal. Included in the gas-related activity is the conversion of coal to a synthetic gas which, in turn, could be used as a feedstock for manufacturing liquid fuels or chemicals or for use in a fuel cell.

The direct utilization priority includes technologies for using coal in a more economical, efficient, and environmentally acceptable manner. This involves the development of various control systems for cleaning coal or removing pollutants from the combustion gases and research into improved combustion concepts.

Throughout this budget, we have also recognized that the Federal Government has traditional responsibilities relating to environmental protection, health and safety. Therefore, a significant portion of our budget emphasizes research intended to mitigate the environmental problems associated with new fossil energy technologies.

Clearly, the Federal Government cannot support research and development on all promising technologies. We have had to make hard choices on which programs to continue and which to deemphasize or eliminate. The fact that a technology is not being supported does not mean that past work in it was not useful; instead, it means that when balanced against other priorities and when considering the incentives now in place for private initiatives, our judgment was that other technical areas were more appropriate recipients of Federal dollars.

INCENTIVES FOR NEW TECHNOLOGIES

Although not specifically part of the budget being discussed today, important aspects of the overall national energy program are the economic, regulatory and tax incentives developed by the Administration to encourage increased production and more economical use of energy. Because they are expected to provide the incentives necessary for increased private sector development of new fossil energy technologies, they form the starting point for a discussion of the Federal program in fossil energy research and development. These incentives can be summarized as follows:

Decontrol of energy prices: On January 28, 1981, the President completed the elimination of Federal price and allocation controls on U.S. oil production and marketing. The drilling industry has responded to the increased economic incentives brought about by decontrol by pushing drilling rates to record levels. In 1981, drilling in the U.S. reached an all-time high, climbing nearly 26 percent over the previous record year of 1980. Oil drilling accounted for the bulk of the increase. While the increased activity in domestic oil fields has not yet revealed another Prudhoe Bay or another East Texas, it has flattened a production trend that had curved downward for nearly a decade. Also, oil price increases have made other energy technologies, including coal combustion, economically attractive in comparison with oil. Regarding natural gas, while we continue to believe that changes to the Natural Gas Policy Act of 1978 are an essential component of a sound energy policy, the President has concluded that asking Congress to accelerate the current schedule of deregulation would overload an already-heavy legislative agenda this year.

Leasing reform: The Federal government owns or controls up to 60 percent of all U.S. energy resources, including up to 80 percent of all known coal reserves in the western U.S. A comprehensive effort to reformulate Federal leasing policies is underway at the U.S. Department of the Interior to guarantee that these energy resources can be explored and produced at a pace consistent with our national energy needs, social and environmental concerns, and the public interest.

Tax incentives: The Economic Recovery Tax Act of 1981 contains several provisions that will assist the private sector in taking a more aggressive role in energy development and production. A 25 percent research and development tax credit is designed to help restore America's technical edge by reducing the after-tax cost of R&D programs and expanding R&D expenditures. This credit may prove to be a successful market alternative to direct government grants for development of the same technologies. The accelerated cost recovery system is expected to stimulate capital investment of all forms. Petroleum production and refining equipment can be depreciated over five years. Some public utility property would get 10-year depreciation; public utility property with useful lives longer than 25 years would be limited to 15-year cost recovery.

Synthetic Fuel Incentives: On February 9, 1982, the President declared the Synthetic Fuels Corporation to be fully operational. This means that more than $14 billion in financial assistance will be available for synthetic fuel facilities. Last fall, the President approved loan and price guarantees for subsidiaries of the TOSCO Corporation and Union Oil, respectively. Each firm is involved in the construction of a commercial-scale oil shale facility. A loan guarantee also was approved for the Great Plains Gasification Associates commercial-scale coal gasification plant. Taken together, these actions are expected to give private companies the assistance they need to begin a new domestic synthetic fuels industry.

THE FY 1983 FOSSIL ENERGY R&D BUDGET

The FY 1983 fossil energy budget proposes new funding for two of the three primary elements which have comprised past budgets. No funding is requested for the Gas program. The FY 1983 budget request is summarized below and compared with the corresponding appropriation in FY 1982 (in millions of dollars):

	FY 1982 Appropriation	FY 1983 Request
Coal	$ 366.8	$ 90.8
Petroleum	40.9	16.1
Gas	9.2	...
Total	$ 416.9	$ 106.9

Since the proposed FY 1983 program involves a major reduction in the scope of fossil energy R&D activities, including a 55 percent cutback in personnel, the Administration will also propose a deferral and redirection of FY 1982 and prior year appropriations to ensure that adequate funds are available for these transition expenses. The Administration will propose deferral of $44.9 million in FY 1982 and prior year appropriations, with the expectation that the deferred amounts would subsequently be used to finance the following transition activities:

- consolidation of the five Energy Technology Centers (ETCs);
- the required reduction-in-force expenses at the ETCs;
- the termination of ETC contracts, where there are termination costs;
- the placing of various federally-owned pilot plants or test facilities associated with the ETCs in a "mothball" status; and
- the disposition of Federally-owned facilities.

The amount proposed for deferral and subsequent reprogramming is based on beginning many of these actions in April, 1982.

COAL

The abundance of U.S. coal — nearly 475 billion tons is the demonstrated reserve base according to the Department's revised estimates released last year — makes it one of the nation's strategic resources in both our energy and economic future. It has also become increasingly significant as other countries continue to turn toward a coal-rich America as a competitive, long-term and reliable source of energy.

The 1200-percent rise in world oil prices in less than a decade has imposed a substantial burden on the economies of oil-dependent nations including the U.S. But it also has created an unparalleled opportunity for America's coal production and utilization industry. The average fuel price difference between coal and oil delivered to U.S. utilities in 1973 was about $2 per barrel of oil equivalent in favor of coal. As of September, 1981, coal's domestic price advantage over oil had widened to more than $20 per barrel.

These inherent economic advantages, coupled with the recently-enacted tax incentives and the activities of the U.S. Synthetic Fuels Corporation, are expected to stimulate increased private sector development of new coal technologies. Therefore, the Federal program will concentrate on acquiring a better understanding of the fundamental behavior of coal as it is burned directly or converted to synthetic fuels and on improving significantly the current efficiency, economics, and environmental effects of coal use.

The proposed Coal program budget for FY 1983 is $90.8 million, a $276 million decrease from the FY 1982 appropriations. The components of this budget request are as follows:

Mining Research and Development: (FY 1982 Appropriation: $ 10.992 million; FY 1983 Request: $ 1.000 million)

	FY 1983 Request (in millions)
Underground coal mining....................	$ 1.000
Surface coal mining	...

The long-term development of advanced underground coal production systems and techniques has been shifted from primarily a contract effort to a largely in-house research program. The Administration will propose transfer of the DOE Mining R&D program to the Department of the Interior's Bureau of Mines which conducts similar research activities. This year, the necessary project reviews and facility and equipment inventories will be completed with the Bureau. Previous research efforts conducted by private firms under DOE contract will be closed out or phased down except for those proposed for continuation in the course of discussions with DOI.

Activities continued in FY 1983 will assess the technical feasibility of advanced and novel mining concepts and on previously-funded, long-term trials of advanced longwall mining techniques.

Longwall mining, although accounting for only 5 percent of the nation's underground coal production, nevertheless offers the potential for significant improvements in productivity, worker safety, and the capability to mine deep and multiple seams. These improvements will become increasingly important as shallower deposits are exhausted and miners must move deeper underground and face more difficult conditions. The Federal program in FY 1983 will concentrate on determining the forces and loads on key longwall equipment, using both underground trial data and the program's unique Mine Roof Simulator located at the Pittsburgh Energy Technology Center. Studies will also be made on the systems which guide the longwall shear across the coal face in an effort to overcome current control and alignment problems.

In the room-and-pillar area — the mining technique accounting for 95 percent of the Nation's underground coal production — the Federal effort will continue assessing means by which advancements in the roof-bolter, miner-bolter, and continuous haulage system can be better integrated.

In past budgets, coal preparation activities were part of the Mining R&D program. In FY 1981, the coal preparation program was combined with the Advanced Environmental Control Technology program under the heading "Control Systems." The coal preparation program would remain within the Control Systems activity in FY 1983 and is not proposed for transfer with the Mining R&D program.

Liquefaction: (FY 1982 Appropriation: $ 98.784 million; FY 1983 Request: $ 26.200 million.)

	FY 1983 Request (in millions)
Direct Hydrogenation	
H-Coal pilot plant	$ ...
Solvent Extraction	
Exxon Donor Solvent pilot plant......	...
Advanced Process Development	
Disposable catalyst	4.200
Process research/technology development.........................	9.600
Indirect Liquefaction.................	8.400
Support Studies/Eng. Evaluations	
Environ. studies & other support	4.000

Liquids remain a primary source of national concern about energy. Although significant potential exists for discovering new sources of conventional petroleum or increasing production from known oil reservoirs, the magnitude and geographical diversity of the nation's coal deposits makes it highly likely, and perhaps necessary, that this resource eventually will be used to increase the supply of liquid fuels. Today, several commercial projects that would convert coal to liquid fuels are being considered by the U.S. Synthetic Fuels Corporation. Most would rely on the "indirect" liquefaction process, where the coal is first gasified, then the gases catalytically recombined to form liquids.

Direct liquefaction — converting coal to liquids without the initial gasification step — offers the potential for improved efficiencies and lower costs. Completion of the government's programs at the 200-ton per day H-Coal and the 250-ton per day Exxon Donor Solvent direct liquefaction pilot plants in FY 1982 will provide a sound technology base for continued development and scale-up of these processes for commercial use by the private sector.

The focus of FY 1983 activities will be on exploring new concepts offering significant improvements in efficiency and economics compared to the H-Coal and Exxon Donor Solvent processes. Information learned from these activities could form the basis for new generations of synthetic fuel technologies in the 1990s and beyond. Much of the Federal direct liquefaction program will concentrate on advanced process development to attain higher thermal efficiencies by reducing the amount of steam, oxygen and/or hydrogen required in the conversion process. One way to do this is by improving the effectiveness of catalysts and minimizing the dependence on high temperatures to break apart the coal structure. By developing cost-effective ways to use these catalysts, the reactivity of the feed coal can be controlled more precisely and undesirable trade-offs in plant design and operating efficiency can be avoided. Improved operating efficiency and better yields of fuel products could be achieved by future plants as a result of this research.

Another approach being pursued in FY 1983 is to separate the step in which coal is dissolved from the step in which the coal extract is upgraded to a higher-quality liquid -- an innovative technique called "Integrated Two-Stage Liquefaction." Bench-scale tests have shown that product yields can be increased by 25 percent over the direct liquefaction processes now at the pilot scale with higher quality liquids produced and less hydrogen being consumed. One of the major efforts in this activity will be to investigate a low-temperature second step that could prolong catalyst life while improving the recycle solvent needed for the dissolving step and to transport hydrogen to the coal extract.

No new funding is provided in FY 1983 for the Wilsonville, Ala., liquefaction facility, a 6 ton-per-day unit owned and operated by the Electric Power Research Institute (EPRI) and a subsidiary of the Southern Company. Sufficient funds, however, are expected to be available from FY 1982, to continue long-range liquefaction research at the facility. DOE funds will be supplemented by funding from EPRI during 1983.

In the indirect liquefaction area, a combination of laboratory and bench-scale efforts will concentrate on new methods of converting the carbon monoxide and hydrogen from coal gasifiers into alcohols or liquid hydrocarbons. A broad-based catalyst and process R&D program is now in place which could result in significant advances in the production of high quality gasoline and/or middle distillates with yields 20-30 percent higher than those of conventional methods. Testing will also be done on new reactor designs that enable closer control of the reaction temperature and more effective recovery of the heat released by the reaction. Research on these new concepts could lead to cost savings of up to 40 percent compared to existing indirect technology.

Transition to the FY 1983 funding level will require eliminating the La Porte, Tex., liquid phase synthesis unit. This facility would have tested alternative methods for producing alcohols and liquid hydrocarbons from gas typical of that from advanced coal gasifiers. Termination of this activity is an example of the judgments necessary when technologies with varying degrees of potential were balanced against the need to limit Federal spending. Given industry's technical understanding of this technology and the obvious economic incentives which would accrue to firms that perfect commercial processes, we believe there are adequate incentives for private industry to continue to develop this process. Our judgment was that more significant benefits could be obtained by investing Federal funds in the development of more effective catalysts and other process improvements.

Also in the indirect liquefaction activity, the internationally sponsored 100 barrel-per-day methanol-to-gasoline (M-Gasoline) pilot plant will be completed in the Federal Republic of Germany in FY 1982. In FY 1983, DOE will collect data on the effectiveness of the advanced "fluidizing" technique employed by the plant to improve temperature control and the effectiveness of the catalyst. Involvement of both the German government and industry and the high probability of royalty payments exceeding the government's investment in the project are the major reasons for proposing to complete this project.

Support studies and engineering evaluations will be conducted to provide independent technical status reviews, engineering assessments, evaluations of ancillary processes (such as hydrogen generation), and environmental and toxicology R&D and coordination.

To achieve a smooth transition from the FY 1982 program to the FY 1983 budget levels, the Administration intends to propose a redirection of FY 1982 appropriations to provide for the orderly termination of selected activities and to provide a source of financing for redirecting program activities. The specific transition actions proposed for FY 1982 are:

- Terminate Federally-funded operations at the H-Coal pilot plant in November 1982 rather than in December 1982 if an agreement can be reached to transfer the plant to the industrial partners; otherwise terminate operations in June 1982 rather than in July 1982 and begin dismantling the plant. Either action would permit deferral of $3 million to be used in subsequent reprogramming proposals;
- Terminate government funding of the Exxon Donor Solvent pilot plant operations on July 1, 1982. This action will permit the deferral of $3.1 million for subsequent reprogramming;
- Terminate the liquid phase methanol process development unit operations in mid April, 1982, permitting $2.225 million to be used for later reprogramming;
- Terminate the SRC-I demonstration plant project design following the expected delivery of the project baseline cost and schedule to Congress in May. Disposition of funds remaining from the $129.6 million deferred from FY 1981 will be addressed by a subsequent Administration proposal to Congress.

Surface Coal Gasification: (FY 1982 Appropriation: $ 53.088 million; FY 1983 Request: $ 10.500 million)

	FY 1983 Request (in millions)
Advanced Process Development	$ 4.250
Systems Engineering Concepts	
Gasifier systems	...
Equipment/instrumentation	3.000
Environmental and Engineering Analyses	
Environmental studies	3.250
Engineering analyses/support	...

To ensure that new concepts for converting coal into synthetic gas continue to enter the development cycle and that problems generic to many gasification processes are overcome, the Administration has established three primary objectives for its surface coal gasification program:

(1) explore and determine the applicability of alternative chemical processing principles and fluid dynamics as possible ways to improve the overall efficiencies and economics of coal gasification;

(2) develop new and promising approaches through the "proof-of-concept" stage for medium-Btu gas that could lower product costs by at least 20 percent as compared to first-generation processes. Medium Btu gas would be emphasized because of its wide diversity of applications including industrial and electric utility uses, conversion to a variety of liquid fuels as well as to synthetic natural gas, or for use as a feedstock for manufacturing chemicals; and

(3) compile a data base on environmental, worker health, and occupational safety performance of commercial and pilot-scale gasification systems and, based on the results, develop effluent treatment concepts capable of meeting existing emission limits as well as workplace safety and health requirements.

A comprehensive technology data base for new gasifier concepts and systems will be established under the advanced process development budget category. Included in the data will be information on process fundamentals, results of new waste water treatment techniques, computer modeling of gasification reactors, and the collection of basic information on all phases of coal gasification, from coal feed through gas cleanup and effluent disposal. The data will include test results from small entrained and fluidized bed reactor systems and could offer the basis for significant long-term improvements over conventional fixed bed gasifiers such as the dry-ash Lurgi. Particularly important will be data which could give new insights into why some coals cake during gasification as well as a more fundamental understanding and control of sintering, clinkering, and slag formation in various gasification systems. Basic experiments will also be conducted on waste water treatment processes and other environmental control systems associated with coal gasification processes.

Operating a coal gasification facility reliably, economically, and efficiently depends substantially on the availability of suitable equipment, instrumentation and control systems, as well as proper materials and fabrication methods. Previous pilot and smaller-scale operations have shown some inherent drawbacks in relying on equipment and technology from other industrial processes, due primarily to the unique combination of high temperature, high pressure, and the erosive and corrosive environments within coal gasification systems. The focus of the systems engineering concepts activity in FY 1983, therefore, will be to investigate new ways to measure key conditions encountered in a gasification facility, such as solids/gas flow, high temperatures, stream composition, erosion and corrosion in process lines, etc. This work will be performed in laboratory test loops. The effects of slag additives on gasifier refractory life will also be assessed in specially designed laboratory equipment, and generic studies will continue on metal coatings and claddings that can withstand the effects of hot coal gases.

No funding has been requested in FY 1983 for developing individual gasifier systems. Several advanced gasifiers have emerged from the prior Federal program, such as the Westinghouse fluidized bed gasifier, U-Gas, and the Exxon catalytic gasification system. The first commercial-size project, the Great Plains Gasification plant, is now moving forward, and knowledge gained from this facility as well as directed bench scale studies will be sufficient to optimize these new technologies. (The Government's monitoring of the Great Plains loan guarantee in FY 1983 will use funds left unspent from the Alternative Fuels administrative account. The anticipated FY 1983 requirement for administering the loan guarantee is $2.4 million.)

Emphasis was increased in FY 1982 on attaining a better understanding of the environmental aspects of converting coal into synthetic gas, and in FY 1983 that emphasis will continue. As part of the environmental studies effort, a gasification cleanup system will be operated under transient conditions and studies will be made to determine the efficiency losses associated with operating a cleanup system under actual end-use conditions. This is expected to lead to more effective designs of environmental control equipment.

As in the liquefaction program, several actions will be required in FY 1982 to ensure a smooth transition into the reduced program proposed for FY 1983. Specifically, these actions are:

- Terminate the Bi-Gas pilot plant operations beginning in mid April, 1982, and place the facility in a "mothball" status. This will permit deferral of $2.5 million to be used for subsequent reprogramming;

- Terminate fixed-bed gasifier activities at the Morgantown and Grand Forks Energy Technology Centers in mid April, 1982, placing the gasifiers in a "mothball" status, thereby freeing $500,000 for use for subsequent reprogramming;

- Terminate the peat gasification work being performed at the HYGAS pilot plant and place the facility in a "protected stand-by/mothball" status, permitting $100,000 to be subsequently reprogrammed;

- Terminate the agglomerating fluidized bed development effort at the Westinghouse process development unit facility in mid April, 1982, and place the facility in a "mothball" status. This action will permit deferral of $2.9 million for later reprogramming; and

- Terminate the atmospheric entrained-bed slagging gasifier (Combustion Engineering) pilot plant program in mid April, 1982, and place the facility in "mothball" status. This action will permit the deferral of $200,000 to be used in a subsequent reprogramming proposal.

Underground Coal Gasification: (FY 1982 Appropriation: $ 8.256 million; FY 1983 Request: $ 0.700 million)

	FY 1983 Request (in millions)
Gasification Technology Development	$ ...
Environmental and Advanced Research	.700

Techniques that gasify coal underground are particularly applicable to coal seams that are too deep and thick or that are pitched at too great an angle for effective mining. Synthetic gas produced from this technique can be converted into transportation fuels such as methanol and gasoline, used as a fuel for electric power generation or as a feedstock for chemical manufacturing, upgraded to synthetic natural gas, or at some specific sites, as a generator of CO_2 for enhanced oil recovery and hydrogen for refinery use or shale oil upgrading.

In FY 1982, the concept of gasifying steeply dipping beds and producing a medium-Btu gas was proven at a site in Wyoming, and large block tests from a Washington coal seam will soon be completed to determine cavity growth and coal response. As a result, the need for futher Federal support of technology development has been diminished, and further technology development is expected to be undertaken by industry. Consequently, the Federal effort will concentrate on legally-mandated environmental monitoring and research at past test sites.

Advanced Research and Technology Development: (FY 1982 Appropriation: $56.256 million; FY 1983 Request: $19.800 million)

	FY 1983 Request (in millions)
Processes	$ 5.000
Direct Utilization	4.200
Materials & Components	
Materials	3.940
Components	.860
Program Development & Coordination	
Budget & administration (construction)	2.800
Environmental activities	2.000
Planning & policy	1.000
University coal research	...

The Advanced Research and Technology Development (AR&TD) program is aimed at extending the fundamental scientific and engineering knowledge base for fossil energy. In this activity, a better understanding is gained of the fundamental mechanisms of converting coal to gas and liquid fuels or burning it directly. Also included are generic studies of materials and components which may have application across the spectrum of fossil energy use. In conducting these activities, scientists will hopefully develop ideas for new processes and possible technical breakthroughs in coal conversion and utilization.

The AR&TD program differs from other activities in the Coal program in that it addresses a broader technological spectrum than research carried out under the coal liquefaction, gasification or combustion categories, for example. Yet, projects supported by the AR&TD budget remain closely connected to long-range Fossil Energy objectives, thus distinguishing the program from the more general mandate of the agency's Office of Energy Research.

Out of this activity, for example, has come a better understanding of coal chemistry, the structure of coal, and the role mineral matter plays in coal conversion. Innovative approaches for upgrading low or medium Btu gas to methane have emerged from AR&TD research, as did the pioneering work establishing the early viability of coal-water mixture combustion. The diversity of the research is reflected by the fact that this activity has also included studies of the mechanisms of soot formation from the burning of synthetic fuels, the identification of critical composition and microstructural aspects of ceramics, and the exploration of new approaches to recovering useable heat from effluent gas streams.

In FY 1983, the processes budget category will include work in both liquefaction and gasification. Small scale research in the liquefaction area will include studies of the generic mechanisms of coal liquefaction, in addition to research on advanced catalysts. Other projects will investigate the effects of mineral matter on the kinetics of coal liquefaction. The gasification activity, likewise, includes pioneering research across a broad technical front involving kinetics, thermodynamics, catalytic mechanisms, modeling, hydrogasification, pyrolysis, fluidization studies, acid gas removal, hot gas cleanup, gas separation, conversion studies, and novel concepts for generating synthetic gas by reaction with steam, air, and oxygen. These activities will generate important data which will be used to construct a sound fundamental synthetic fuels technology base

The direct utilization effort will expand the technology base required for the increased use of coal including low-rank coals, chars, beneficiated coals, and coal-liquid mixtures. Approximately 50 individual efforts will address areas such as fuel injections and preignition processes, flame chemistry, pollutant formation and control, fouling and slagging, and modeling and scaling techniques. Emphasis will be given in FY 1983 to developing a data base required for retrofitting oil and gas designed equipment for coal firing and expanding coal firing options in industrial process heat applications.

The materials activity will provide the data for future decisions on structural materials in fossil energy conversion systems and could lead to materials with higher efficiency and greater longevity. The program will attempt to gain a better understanding of the basic phenomena involved as materials degrade in fossil energy systems.

The components program will continue fundamental research into the peripheral and support equipment involved in fossil energy utilization. Specific research will focus on advances in analytical models of flow patterns when solids, liquids and gas are mixed (called "three phase flow"), the development of a thermodynamic base for heat exchangers and the development of scaling parameters for multi-phase flow systems.

The funding request for budget and administration includes $1 million for incremental funding of the construction and initial equipping and operation of a surface water containment and waste water treatment facility at the Pittsburgh Energy Technology Center so that current violations of waste discharge regulations can be corrected. The remaining $1.8 million is for a waste water treatment facility at the Morgantown Energy Technology Center and for other general plant projects at the Energy Technology Centers. The Department is currently planning a major consolidation of the Fossil Energy field structure, aimed at reducing the number of Energy Technology Centers from five to perhaps one or two. If, as a result of these actions, these general plant projects are not required, an amendment will be forwarded to the Congress.

Funding for environmental activities will finance policy development, R&D assessment and compliance overview in the areas of environment, safety, and health. This activity will ensure that fossil energy projects are conducted in compliance with relevant laws and regulations, such as the National Environmental Policy Act, the Clean Air Act, the Occupational Safety and Health Act, and others. By involving environmental, health and safety concerns early in the technology programs, the future acceptability of new process concepts emerging from the Fossil Energy program can be assured.

Planning and policy funding will be used to conduct contract studies and data collection on a variety of fossil energy related topics which will be valuable for planning future research and development activities. Administrative support for the Fossil Energy program is also provided under this budget category.

No funds have been earmarked specifically for university activities in FY 1983 since government-supported university projects can be found throughout the Fossil Energy program and in other Departmental programs.

Combustion: (FY 1982 Appropriation: $ 30.544 million; FY 1983 Budget Request: $6.500 million)

	FY 1983 Request (in millions)
Atmospheric Fluidized Bed Combustion	$ ...
Pressurized Fluidized Bed Combustion	
IEA Grimethorpe PFB	2.500
PFB Technology Base Support	2.000
Alternative Fuel Utilization	
Alternate fuel mixtures	2.000

Industry has now assumed the lead in developing and marketing several new coal combustion techniques, including atmospheric fluidized bed combustion systems. At least 35 atmospheric fluidized bed boilers are now in operation, under construction, or being designed, and at least 10 boiler manufacturers are now developing or marketing commercial versions of this new combustion technology.

As a result, the Federal combustion research program in FY 1983 will concentrate on technology base research for pressurized fluidized bed (PFB) combustion and support for the international experimental pressurized fluidized bed facility at Grimethorpe, England. The pressurized fluidized bed approach represents the next generation of advanced coal combustors. The International Energy Agency's Grimethorpe program will focus on evaluating combustor performance, coal feeding, ash removal, and gas stream cleanup. This project has special significance because of the international cooperation it entails with Great Britain and the Federal Republic of Germany. Construction of the unit has been completed and the first test runs have taken place. The $2.5 million requested in FY 1983, along with funding from our international partners, is expected to be sufficient to develop a sound data base for the pressurized fluidized bed combustor, the heart of this advanced combustion concept. We will also continue to assist in efforts to interest other governments or industries in sponsoring the follow-on phase of the project which would include modifying the facility to incorporate a gas turbine.

While no funding is requested for the Wood-Ridge, NJ, pressurized fluidized bed facility, construction is continuing in FY 1982 and negotiations will be undertaken to reduce Federal involvement while still completing the construction and operation of this facility.

The PFB technology base support will concentrate on the basics of fluidized bed combustion at pressures of up to 20 atmospheres. Corrosion and erosion of various boiler and gas turbine materials will be studied. Scale-up laws will be better defined. An investigation of the effects of tube bundle solidity on heat transfer and studies of the combustion characteristics of coal-based mixtures and low rank coals will be conducted.

Industry has also assumed the initiative in commercializing coal-oil mixtures, in some cases using data generated in past government research programs. The alternative fuel utilization program, therefore, will focus on higher-risk coal-based slurry fuels, such as coal-water and coal-water-alcohol. Coal-water mixtures, particularly, have attracted substantial interest recently because of their potential for replacing 100 percent of the oil burned in a utility or industrial oil-designed boiler. Mixtures of up to 70 percent coal and 30 percent water have been shown to have storage, handling, and transportation characteristics compatible with conventional oil-handling equipment with only modest modifications. The Department recently selected two firms to perform a series of tasks intended to build a sound data base for industry to make future commercial decisions on coal-water mixtures. If this data, coupled with information developed by an on-going in-house research program, is successful in stimulating industry to move ahead on its own, as much as 3 million barrels per day of oil could be displaced in major fuel burning installations across this country.

Changes in the FY 1982 Combustion Systems program are also being proposed to provide a source of funds for the transition from FY 1982 to FY 1983. Specifically, these proposed actions are:

- o Renegotiate the contract for the PFB Combined Cycle pilot plant project at Wood-Ridge, NJ, to increase private industry funding for construction completion and operations, thereby permitting $6 million in Federal funds to be subsequently reprogrammed;
- o Terminate the PFB alternate turbine test activities, permitting the deferral of $700,000 for later reprogramming;
- o Terminate the advanced combustion technology program, which would permit deferral of $1.056 million for subsequent reprogramming.

Control Technology and Coal Preparation: (FY 1982 Appropriation: $ 25.152 million; FY 1983 Request: $ 8.600 million)

	FY 1983 Request in millions
Coal Preparation & Analysis	$ 2.000
Flue Gas Cleanup	
Combined flue gas cleanup	3.000
Gas Stream Cleanup	
Turbine cleanup systems	1.500
Fuel cell cleanup systems	1.200
Process modification technology	.500
Cleanup Base Technology	.400

The economics of producing energy from coal are penalized by sulfur and ash-forming impurities in the coal. Added costs are incurred in transportation, capital investment, operation and maintenance, impaired plant reliability, environmental control, and waste disposal. Today's commercial coal users are limited in their methods of removing these impurities. Significant benefits could be gained if future plant operators could make tradeoffs between a wider variety of coal preparation methods applied before coal is shipped, and improved combustion gas clean-up systems installed at the plant site. To assist industry, therefore, the Federal R&D program has been restructured to treat the entire coal cleaning process — from mine to stack — as successive, interdependent steps.

The first step in this "control systems" approach is to remove sulfur and ash from coal by precombustion cleaning, or coal preparation. The more of these impurities that can be removed, the greater the potential savings in transportation, power generation, and environmental costs. Many U.S. coals resist extensive cleaning unless they are ground considerably finer than present day technology allows. Our goal is to develop techniques to grind coal as much as 60 times finer than is common in today's coal preparation plants. Once this is done, fine coal physical cleaning can remove the bulk of ash-forming minerals and the mineral sulfur or iron pyrite in coal. An even higher quality product can be obtained by extracting most of the remaining organic sulfur (which is chemically bound to the coal structure) and residual mineral matter by chemical cleaning.

The Federal program addresses these challenges. Our efforts are intended to aid the private sector in developing methods to clean even inferior U.S. coals to qualities approaching fuel oil. Our research to date gives us strong encouragement that this can be done. If we establish the necessary firm technology base and validate cost-effectiveness through creditable "proof-of-concept" engineering tests, the private sector will move ahead to full commercial development. Two advanced chemical methods, microwave desulfurization and fused salt treatment, appear particularly promising and development of both will continue in FY 1983.

With industry assuming much of the initiative in moving new flue gas desulfurization technologies — or "scrubbers" — into commercial practice, the Federal program is now focusing on a significantly more advanced generation of flue gas cleanup systems. Emphasis is being placed on electrochemical or chemical systems which simultaneously remove both NO_x and SO_2 in a single reaction, rather than in a series of separate steps as is the practice today. The FY 1983 budget would continue research on electron beam irradiation and advanced chemical processes begun in 1981. University-level testing of advanced particulate control will also continue.

To improve the prospects for new combined cycle generating systems powered by synthetic gas from coal or the exhaust from a coal-fired pressurized fluidized bed combustor, government research will also continue in turbine cleanup systems. Efforts in FY 1983 will continue projects begun in prior years to improve methods for removing alkali and particulate impurities from hot (up to 2400 degrees F), high pressure (500 psi) gas streams before they can damage conversion cycle equipment and/or violate environmental emissions standards. "Proof of concept" tests will be conducted under actual pressurized fluidized bed combustor conditions using a variety of coals.

The aerodynamics/materials problems associated with high-speed turbomachinery do not exist when coal gasifiers are linked to the molten carbonate fuel cell. The fuel cell, however, is susceptible to electrode poisoning and/or deterioration, and electrolyte contamination. Therefore, the Federal control systems program also includes a fuel cell cleanup systems activity to quantify the limits of contaminants acceptable to the molten carbonate fuel cell, to evaluate the applicability of state-of-the-art gasifier cleanup systems to fuel cells, to determine the incentives for developing high temperature fuel cell cleanup processes, and to build the technology base so that industry can develop the most appropriate cleanup systems.

An alternative to flue gas cleanup or the use of separate cleanup equipment for hot gas cleanup may be to modify the combustion or gasification process itself to either suppress pollutant formation or remove the pollutant as it is formed in the gas. This approach is being pursued under the process modification technology budget category. To determine the most desirable approaches, a better understanding is needed of the mechanisms by which pollutants are formed along with their chemical make-up. Once specific control techniques are conceived, they must undergo technical and economic feasibility tests to determine the requirements for further development. In FY 1983, an assessment of the most desirable NO_x control techniques, centering on combustion modification and catalytic combustion, will be completed. Guidelines for selecting physical/chemical techniques for SO_2, alkalies, trace metals, and hydrocarbon capture or destruction will also be determined.

The cleanup base technology activity has been revised to focus on combustion system waste management and characterization, and will emphasize two projects: waste sampling and characterization, and assessments of the Resource Conservation and Recovery Act and other environmental regulation impacts on coal-fired utilities.

Fuel Cells: (FY 1982 Appropriation: $34.464 million; FY 1983 Request: $ 9.500 million)

	FY 1983 Request (in millions)
Phosphoric Acid Systems Development.........	$ 5.000
Molten Carbonate Systems Development........	3.500
Advanced Concepts...........................	1.000

Because they generate electricity and heat by electrochemical reactions, and not by combustion, fuel cells have particularly attractive environmental, efficiency, and siting flexibility benefits. Applications range from electric utility multi-megawatt power plants to on-site multi-kilowatt cogenerating systems to power for transportation and defense facilities.

During FY 1982, power will be produced from the nation's first prototype utility fuel cell, the 4.8 megawatt phosphoric acid demonstrator power plant in New York City, and development will continue on critical technology components and subsystems required to improve system life, power density, and costs. Also, the first field tests of the 40-kilowatt on-site power plants will begin, and the first 40-kilowatt systems will be delivered under a joint DOE/Gas Research Institute field test program. The FY 1983 phosphoric acid systems development program will complete the retrofit of the fuel cell stack for the 4.8 megawatt power plant and will provide funding for completion of development activities for the electric utility technology development program. Phosphoric acid fuel cell technology has reached the stage where the private sector can now begin assuming responsibility for manufacturing and marketing the technology.

Second generation molten carbonate fuel cell systems development will also continue in FY 1983 with emphasis continuing to be placed on critical technical issues. While both phosphoric acid and molten carbonate fuel cells can be linked to a coal gasifier, a particular advantage of the molten carbonate system is that it can be combined with a steam or a gas turbine thereby boosting the system's overall efficiency. In FY 1983 a selected stack of sub-scale size molten carbonate fuel cells will be operated, a commercial-type cell will complete long-duration test operations totaling 10,000 hours, and an evaluation will be undertaken of the capability of the system to operate on coal-derived gas.

The advanced concepts program focuses on the solid oxide fuel cell, a design concept which offers the advantages of solid state stability and can be more easily manufactured. The funding requested in FY 1983 would complete a 5,000 hour test of an advanced design cell, permit preliminary design of 5-10 kilowatt stack module, continue follow-up analysis and development advanced concepts for mobile applications, and complete the initial R&D requirement for solid oxide fuel cell configurations.

Heat Engines: (FY 1982 Appropriation: $ 15.408 million; FY 1983 Request: $ 0)

The Heat Engines program in prior years contained a variety of associated activities, including the high temperature gas turbine cooling and low NOx combustor technology; gas turbine and diesel engine materials and coating development, externally fired heat engine cycle work, and feasibility testing of diesel engines and gas turbines on coal-derived liquids and coal-water mixtures.

These activities are approaching the "proof of concept" point where the private sector is provided the option to continue further development. Many of the technology improvements supported by this program could eventually be undertaken by manufacturers as they make advances in existing technologies. Therefore, no funds have been requested in FY 1983 for continuing this program.

In conjunction with the planned termination of this program, $8.736 million in FY 1982 funds is being proposed for deferral. This funding originally was intended for Phase III of the High Temperature Turbine Technology Program.

Magnetohydrodynamics (FY 1982 Appropriation: $21.840 million; FY 1983 Request: $ 0)

The Administration also proposes to phase out the overall magnetohydrodyamics (MHD) program beginning in FY 1982, and no funds are requested for FY 1983. MHD is a technology that requires a large-scale "proof of concept" demonstration (in the range of a 250 to 300 thermal megawatt facility) with attendant development costs approaching perhaps $500 million. In light of budgetary constraints, the multiple energy conversion technologies presently being pursued by both private industry and the Fossil Energy program, and the relatively high development costs and relative expected benefits of MHD, the Administration cannot support continuation of funding for this technology.

Under this budget proposal, termination actions would begin in April 1982. The advanced power train development program would be brought to an orderly conclusion during FY 1982 and serve as the focal point for consolidating the key technical data which may subsequently be of use to government and private industry. The Component Development and Integration Facility in Butte, Montana, and the Coal-Fired Flow Facility in Tullahoma, Tennessee, would be placed in a "mothball" status. By beginning these actions in April, sufficient funds would be available to fund termination liabilities and facility mothball costs, plus defer an additional $1.9 million for subsequent reprogramming.

PETROLEUM

The target for recovering new supplies of oil in the U.S. is extremely large. Nearly 450 billion barrels of oil have been discovered, yet only one-quarter of that has been produced. Best estimates are that 28 billion barrels of reserves remain to be produced through conventional recovery methods. To date, the contribution of enhanced oil recovery has been roughly 400,000 barrels per day, nearly three-fourths of which is produced by steam injection in heavy oil reservoirs. Over the longer-term, however, enhanced oil recovery is expected to increase, particularly with the incentive of decontrolled oil prices. The focus of the Federal research effort, therefore, is to concentrate on more generic technology base research, leaving to industry the task of bringing technology to commercial readiness.

Oil shale is another potential source of unconventional petroleum supplies. Several retorting processes are considered commercially feasible today. The companies involved in oil shale projects have the financial and technology resources to commercialize existing and new technologies. Therefore, the government's efforts will concentrate on generic research on the chemistry and physics of western oil shale processing and on explosive fracturing. Work will also continue on the potential environmental effects of oil shale development and their mitigation.

Enhanced Oil Recovery (EOR): (FY 1982 Appropriations: $ 16.176 million: FY 1983 Request: $5.8 million)

	FY 1983 Request (in millions)
Heavy Oil	$ 1.9
Light Oil	3.9
Tar Sands	...

Because private industry can be expected to continue to improve existing EOR processes and eventually to develop new concepts in response to oil price increases, the Federal EOR program is now directing its primary emphasis on long-range, high-risk technology base research that attract limited private capital.

The heavy oil program will continue to concentrate on improving the recovery efficiency of thermal processes. If recovery efficiency can be improved from the current 50 percent to a level of 60 percent, an estimated two to three billion barrels of oil could be added to the nation's recoverable reserves. Similarly, if heat losses can be reduced in wellbores, thereby permitting steam to be piped to deeper reservoirs, domestic reserves could be boosted by one to two billion barrels. In FY 1982, the long-term test of the downhole steam generator, part of Project Deepsteam, will be completed. The significant private sector interest in this concept has eliminated the need for additional development by the Federal government. The FY 1983 funding request, therefore, provides for continued research into the behavior and mechanisms of heavy oil recovery. Research would continue to investigate flow properties, the kinetics of in situ combustion, and steam mobility control. Evaluations of chemical additives with steam and studies of foams and other ancillary materials for improving sweep efficiency, mobility control, and environmental effects would also continue.

The light oil funding request, likewise, would continue fundamental research into the behavior and mechanisms of light oil EOR processes, including chemical and miscible gas flooding. Research will be conducted into mobility control, the interfacial tension effects between oil, chemical, and water molecules, the stability of chemicals at the elevated temperatures and increased brine salinity encountered underground, basic studies on how oil drops coalesce, and in several other areas. The research will be conducted in-house and at several universities, and several ongoing, previously funded, cost-shared field tests will continue to be monitored.

The private sector has adequate incentive to continue development of tar sands in view of the current state of the technology, the decontrol of oil prices, the existence of the Synthetic Fuels Corporation, and recent regulatory changes allowing tar sand leasing on Federal lands. Therefore, no further federal R&D is warranted and no funding has been requested for FY 1983. Activities related to the in situ tar sand experiments at Vernal, Utah would be phased down and concluded in FY 1982, permitting the deferral of $500,000 to be used for subsequent reprogramming.

Advanced Process Technology: (FY 1982 Appropriation: $ 4.032 million; FY 1983 Request: $ 3.000 million)

	FY 1983 Request (in millions)
Advanced Exploratory Research	$ 3.0

A variety of generic and fundamental supporting research activities related to oil, gas, oil shale, and tar sand resources are conducted under this budget category. For example, a portion of the Advanced Exploratory Research budget will be continued on the use of microbes for enhanced oil recovery. Researchers will attempt to identify and isolate microbial strains that can survive and generate through a biological process, surfactants, polymers, emulsifiers and other chemicals under reservoir conditions. Research will also be done on advanced recovery methods using vacuum extraction, superheated water, and novel solvent products. Studies to remove mutagenic, carcinogenic, and other toxic products from heavy crudes, tar sand bitumen, and shale oil will be pursued, as well as supporting research into the stability, degradation mechanisms, and rheology of synthetic fuels. Analytical support will also be provided to the Strategic Petroleum Reserve Office.

A variety of "cross-cutting" research applicable to all petroleum-related programs will also be conducted. This will include efforts dealing with rock mechanics, improved analytical methodology, and instrumentation for studying the physical and chemical properties of fossil derived liquids and for aiding in the extraction of gas from tight reservoirs. Another aspect of this program involves fundamental research into the existence and magnitude of abiogenetic methane and methane hydrates, as well the evaluation of the potential for recovering these unconventional resources.

Oil Shale: (FY 1982 Appropriations: $19.152 million; FY 1983 Request: $6.300 million)

	FY 1983 Request (in millions)
Technology Base..............	$ 3.700
Environmental Mitigation.....	2.600

Oil shale technology is now making the transition from laboratory and pilot scale testing to the first commercial applications. Initial private sector activities are concentrated in the western U.S. where the richest reserves of oil shale lie. Substantial lower quality resources also occur in the eastern states. Together, the oil potential represented by U.S. oil shale deposits is much greater than all of the oil reserves of the Middle East.

As privately-initiated projects continue to move forward, the Federal program will limit its oil shale technology base activities to generic research on the chemistry and physics of western oil shale processing and on explosive fracturing. If a better understanding can be developed in these areas, future oil shale processes could achieve significantly higher conversion efficiencies and fracturing could be done with greater predictability. Examples of this work include the development and application of improved X-Ray, nuclear magnetic resonance, and other analytical techniques for application to shale oil products and chemical reactions. Funding for previous development and demonstration projects, conducted under cost-sharing contracts, will be completed in FY 1982, and the FY 1983 budget level represents the winding down of related evaluation activities, phase out of further technology base research on Eastern shales, and postponement of R&D on specific process concepts.

While the first shale oil production plants will be able to comply with current environmental standards using conventional technology, the long-term accumulation of such facilities in environmentally-sensitive areas could require improvements in environmental control technologies. Therefore, a prime focus of the Government's research program will deal with environmental mitigation. The proposed budget would permit a study of the relationship between process conditions and the efficacy of sulfur removal as well as the chemistry of formation and control of other air emissions. Also, laboratory experiments would be continued to develop solid waste management strategies, including the preparation of an integrated solid waste management plan. Studies would also continue on potential water purification techniques, and a waste pile stabilization plan would be completed for the government-owned Anvil Points facility in Colorado.

Two revisions are also proposed in the FY 1982 budget. Government participation would be concluded in the Geokinetics oil shale project cooperative agreement, thereby allowing approximately $1.8 million to be proposed for deferral and subsequent reprogramming, and the solicitation for proposals involving innovative eastern shale conversion concepts would be cancelled, permitting $624,000 to be used as a source of funds for subsequent reprogramming.

GAS

Natural gas currently supplies about one quarter of the total energy consumed in the U.S. While the annual addition of gas reserves from new discoveries has been less than withdrawals from proved reserves for almost the last decade, the difference between additions and withdrawals has decreased steadily as exploration has picked up with phased energy price decontrol.

The Federal research role in this area has historically concentrated on unconventional sources of natural gas such as eastern shales, western tight sands, and coalbed methane. Industry efforts are now increasing in these areas, and the private sector has both the financial and technical resources to develop these new gas resources in response to market incentives. Consequently the unconventional gas recovery program has been scheduled for termination in FY 1983.

Unconventional Gas Recovery: (FY 1982 Appropriation: $8.712 million; FY 1983 Request: $ 0)

Industry has already begun to develop western tight blanket sands. As that technology evolves to cover the more difficult lenticular deposits, production from this resource should increase. And because there is a strong link between unconventional technology development and economic incentives, Federal activities in this area can begin to phase down. The resource evaluation for eastern Devonian shales is essentially complete and final verification of the results is expected to be done by industry. Thus, all geologic resource assessment activities will be phased down in FY 1982 and will no longer be supported by this program. Previous government-funded activities have demonstrated that methane can be extracted from coalbeds and used both with and without associated mining operations. The industry is better qualified to deal with barriers to commercial production and gas ownership; therefore, no funds are requested for this program in FY 1983.

Work in FY 1982 will be confined mainly to bringing the multiwell test in the western lenticular tight sands to a successful completion. To achieve a smooth shutdown of the Federal unconventional gas program, the Administration is proposing to cancel the third well of the multiwell project and eliminate all research dependent upon that well. Research at the site will be completed in FY 1982 with termination in FY 1983 or transfer of the project to industry. This action will permit $1 million to be deferred in FY 1982 for a subsequent reprogramming proposal.

PROGRAM DIRECTION

The FY 1983 request of $9.0 million for program direction ($8 million for the Coal program and $1 million for the Petroleum program) will provide funds for salaries, benefits and travel for Fossil Energy headquarters personnel during the fiscal year. The funding of these expenses for the Energy Technology Centers is provided in the program activities they support, not from the Program Direction line item.

CONCLUSION

In the past the Federal government has focused heavily on budgetary spending for fossil energy. In focusing on federal spending, the government in our view, failed to deal with the basic problems affecting energy in this country. For example, past policies artificially held the price of oil and natural gas below market levels. This discouraged the use of substitute fuels such as coal and also discouraged energy conservation and further discovery of oil and gas. The alternative approach, proposed by the President, involves a much broader strategy regarding energy and the overall economy. The funding proposed for the Fossil Energy programs should be judged in the context of this broader strategy involving less government spending, regulation, and taxation to achieve a simply energy policy objective -- the encouragement of efficient production and use of domestic energy resources.

The fossil energy budget we have proposed is part of an overall Federal energy budget that will save the American taxpayer nearly $10.5 billion by the end of FY 1983 compared to what the previous Administration intended during the same period. As President Reagan said in his State of the Union message, reducing Federal spending is one of four "commonsense fundamentals" of our Economic Recovery Program.

We ask this Committee to weigh our budget proposal in the same spirit and with the same contructive purpose as was evidenced last year. Through an open dialogue, we hope to show that this budget represents a realistic approach to building a sound knowledge base from which the fossil energy industry can select, further develop, and commercialize the technological options it deems most appropriate to meet our energy needs most efficiently.

Mr. **Fuqua**. Thank you, Mr. Mares.

Mr. Gregg, do you have any questions?

Mr. **Gregg**. There is a lot of legislation moving around the House that deals with the question of sulfur emissions, especially the question in the Northeast is the question of acid rain—the deposition of acid rain as it relates to the use of utilities of high-sulfur coal. Have you folks considered that concern in structuring your budget and structuring the direction in which you are going?

SO_x AND NO_x REMOVAL

Mr. **Mares**. Yes, sir, we have. We have in our programs efforts to develop advanced concepts for the removal of SO_x and NO_x and particulate matter. These advanced concepts are beyond what the private sector is doing today. We hope that those developmental efforts will be successful and add to the ability of our industrial community to reduce those effluents.

We are also supporting the efforts to understand, in greater detail, the aspects of acid deposition which we think are not sufficiently understood to allow us to develop a national control strategy at this stage.

Mr. **Gregg**. What percentage of your budget is going into fossil fuel research that is related directly to reduction of sulfur emissions?

ENVIRONMENTAL ACTIVITIES

Mr. **Mares**. In the limited case that I described, that of the advanced activity concept for SO_x and NO_x removal, we have requested 1983 appropriations of $3 million. That, however, is misleading. Most of our technology work—in fact I would assert that all of it—includes as one of its criteria that it must improve the environmental capabilities of the technologies that we are trying to improve. Although I gave you a number that is very specific to flue gas, we also have other projects underway which are addressed toward environmental problems, whether it is in coal liquefaction, coal gasification, coal-water mixtures, preparation of coal, that is, removal of sulfur from coal before it is burned—all of these programs are directed toward building the technology base for this country from which options will be developed through process development and commercialization by the private sector.

Mr. **Gregg**. You have got a $100 million budget approximately in this area. Of the $97 million which is not being spent specifically on this but which is related to it because it is your overall game plan, what is your biggest item in there that would be involved with acid rain and the mitigation of acid precipitation? We talk about washing.

Mr. **Mares**. Well, sir, we are not involved in improving the current technologies that are available to the utility and industrial sector. That, in our judgment, is the appropriate role for the private sector. Our technology effort goes beyond what industry would otherwise do. It is not something that will come into immediate fruition this year or next year.

In an overall sense, I would say that probably a quarter of our budget is in one way or another directed toward environmental activities, but not specifically to work on acid precipitation. Essentially, in all of them, we are characterizing the materials we are working with and looking for ways to reduce the amounts of sulfur oxides, nitrogen oxides from the combustion of fossil fuels.

Mr. **Gregg**. Would it be possible for you to send my office a memo which would outline the relationship between these activities and how you think they are going to directly impact or indirectly impact emissions?

Mr. **Mares**. I would be pleased to do so.

Mr. **Gregg**. Thank you very much.

[The information referred to follows:]

The primary goal of the Fossil Energy program is to perform generic high risk, high potential R&D to assist the private sector's use of fossil fuels in an economically attractive and environmentally acceptable manner. Therefore environment is a primary consideration in the development of our technology programs both in a direct and indirect approach.

The following programs are being developed to specifically control emissions associated with the use of fossil fuels. These controls consider precombustion, combustion, and post combustion cleanup technologies.

- Coal Preparation FY 83 $2,000K

  The coal preparation program is directed towards the development of advanced coal cleaning technologies required by private industry to meet the anticipated growth for higher quality coal. Since most coals resist conventional cleaning practices either because the ash and sulfur constituents are too finely disseminated in the coal structure or because the organic sulfur is too high, the thrust of this program is directed towards:

  - New and advanced cost effective technologies to clean and condition finely divided coal as necessary to liberate finely dispersed pyrilic sulfur and ash particles.
  - Advanced and unconventional methods to extract organic sulfur from coal.

The goal, therefore is to get 90% or more of the ash and sulfur out of coal by a combination of advanced cost-effective physical and chemical processes.

- Flue Gas Cleanup FY 83 $3,000K

This program focuses on the development of a technology base for cost effective and environmentally superior processes which will remove pollutants from coal combustion stock gases, e.g. SOx, NOx, particulates and hydrocarbons. The flue gas cleanup program seeks to assist the private sector in alleviating the problems associated with the pollutants, which are expected to increase with increased use of coal, by pursuing a program aimed at developing new non-proprietary technologies which provide for the higher and more cost effective removal processes. Simultaneous removal of SOx and NOx by electrochemical or chemical systems in a single reaction offers a potentially more cost effective and reliable means of meeting New Source Performance Standards (NSPS) and State Implementation Plan (SIPS) requirements than most currently used control technology systems. Thus, this approach is the focus of our attention for SOx and NOx control. Coal combustion also results in the emission of large amounts of particulate matter. The primary thrust of this portion of the FGD program is to develop particulate cleanup systems which have increased cost effectiveness, higher efficiency and improved reliability. Attention will also be given to the effectiveness of advanced particulate devices for removing fine respirable particulates.

- **Gas Stream Cleanup** FY 83 $2,000K

a. Turbine Cleanup Systems FY 83 $1,500K

The primary objective of this task is to develop the required technological base to assist industry's development of cleanup systems to be used in conjunction with gas turbines operating on combustion products from a coal-fired Pressurized Fluidized Bed Combustion (PFBC) or fueled with a low or medium BTU coal-derived gas. Primary emphasis is on particulate and alkali removal at elevated temperatures with initial application to the PFBC. The PFBC has demonstrated the capability to meet New Source Performance Standards (NSPS) with respect to NOx and SOx emissions. The performance objective is to develop hot gas cleanup systems by 1985 which are capable of reducing particulate levels to .012 g/SCF and alkali levels to .02 PPM. Prior test results of selected particulate concepts such as advanced high temperature high pressure (HRHP) electrostatic precipitators, ceramic felt bags and granular bed filters will be evaluated and conclusions summarized for future HTHP PFBC combined cycle application.

b. Process Modification Technology FY 83 $500K

A potentially cost effective alternative to flue gas cleanup or the use of separate cleanup equipment for hot gas cleanup is modification of the combustion or gasification process itself to either suppress pollutant formation or remove the pollutant upon formation in the gaseous phase. This approach has applicability to the control of pollutants such as H2S or SO2, NOx, alkalies, trace metals, and organic compounds. The primary approach taken is to study the mechanisms by which pollutants are formed along with their chemical make-up, conceive techniques for their control, and to then determine the technical and economic feasibility of further developing these control techniques. Efforts on NOx control center on combustion modification and catalytic combustion. Work on the other pollutants emphasizes the identification and evaluation of chemical additions, including sorbents, which can capture or destroy the pollutant when introduced directly into the gas stream, with the air required for combustion or gasification, or with the coal itself.

In FY 1983, an assessment of the most desirable NOx control techniques will be completed. In addition, guidelines for the selection of physical chemical techniques for SO2, alkalies, trace metals, and hydrocarbon capture or destruction will be determined.

- Pressurized Fluidized Bed Combustion FY 83 $2,500K

  Pressurization of the fluidized bed combustor is a means of reducing emissions of SO2, NOx, CO, and hydrocarbons while simultaneously effecting an increase in power generation efficiency with the possibility of reducing the cost of power generation.

  In FY 83, testing is being performed at the Grimethorpe Pressurized Fluidized Bed Plant specifically looking at ash removal and gas cleanup process.

There are other components of the Fossil Energy program which are not related directly to the control of sulfur and nitrogen oxide emissions. However, these programs do contribute, indirectly, to the reduction of SOx and NOx emissions because of the clean fuels produced or the inherent nature of the process. These technology programs are listed below.

• Surface Coal Gasification and Coal Liquefaction	$3,250K
• Fuel Cells	$1,200K
• Underground Coal Gasification	$700K
• Oil Shale	$1,525K
• Enhanced Oil Recovery	$1,375K
• Advanced Exploratory Research	$750K

Mr. **Volkmer** [acting chairman]. Mr. Mares, I would like to inquire a little bit first about the deferrals or rescissions—deferrals I guess we had better call them——

Mr. **Mares**. Yes, they are deferrals.

Mr. **Volkmer**. On some of the projects, and, along with that I understand a reduction in force within the agency, within the fossil fuel area, is that correct?

Mr. **Mares**. We have prepared a list of deferrals that we are submitting to Congress. I understand that it has been signed and will be submitted, presumably by the end of this week. It is a formal listing of deferrals relating to a series of projects and activities. It is the same series of projects and activities that have been in the budget submission that was presented to Congress in February.

The expectation is, obviously, that we would take action upon those unless Congress rules otherwise.

Mr. **Volkmer**. My problem is that you are also going to have—it is in your schedule—a reduction in force because of those deferrals, do you not?

Mr. **Mares**. No, sir. There is no reduction in force in fiscal 1982 that is tied to the deferrals.

Mr. **Volkmer**. So, if the deferrals are not agreed to by the Congress, you will still have sufficient personnel to oversee and handle the programs?

Mr. **Mares**. Absolutely. We have an analysis that has been underway since last fall, directed toward improving the overall efficiency of the operation of the Office of Fossil Energy. By streamlining and consolidation of some activities. That study is proceeding and may result in some reductions in force. That is, however, independent of the deferral request.

Mr. **Volkmer**. I would like to ask you briefly on coal liquefaction—direct liquefaction.

With the budget as we have it in 1983 where do we have, let us say, liquefaction testing that will continue? Or, projects that will continue?

COAL LIQUEFACTION

Mr. **Mares**. There is technology base activity that will continue in the area of coal liquefaction. We are working on a two-stage liquefaction process in which there has been a lot of optimism. We can take the current technology base that exists in the Exxon donor solvent and H-coal processes and make improvements upon them. That work can be done at the 6 ton a day pilot plant at the Wilsonville facility. The pilot plant is operated jointly by DOE and EPRI.

Mr. **Volkmer**. I thought you were closing that down?

Mr. **Mares**. No, sir. We have been able to determine that the combination of fiscal year 1982 funds plus some in 1983 from EPRI that the Wilsonville pilot plant activity can be continued at a reduced level of effort, and it will not need to be shut down in fiscal year 1983.

Mr. **Volkmer**. In other words, you will just use it on a pilot scale though?

Mr. Mares. Yes, sir. It is a small pilot plant, about 6 tons a day. The pilot plant is owned by EPRI. EPRI's funding as well as some funding from DOE will allow that pilot plant to be operated in fiscal year 1983 to test at a "proof of concept" level ideas such as the two-stage liquefaction concept of coal liquefaction.

Mr. Volkmer. And the H-coal plant will be what? Dismantled?

H-COAL PLANT

Mr. Mares. No, sir. Our hope is that the H-coal plant will not be dismantled but will be taken over by the H-coal participants at the termination of our contract with the H-coal group. Our obligation, unless it should change, is to dismantle that facility and to clear the site. That does not make good sense in terms of the value the facility has. This is so particularly in the light of the fact that the H-coal participants not only have a project that they have submitted to the SFC but also that, at least the trade journals report, there are discussions underway with potential licensors for that technology; the use of that pilot plant being desirable in the development of those technology licenses.

What we are hoping is, that in the course of negotiating the end of our relationship with the H-coal group that the plant will not be shut down, and that they will take over the responsibility for the final disposition of the plant. It would then be their responsibility to decide whether or not to dismantle the facility.

Mr. Volkmer. Do you have any information yet from the group, Exxon and the rest of them, that they continue on with operations?

Mr. Mares. At this moment the outcome is uncertain. We believe that they are hoping to continue operations.

Mr. Volkmer. But you also believe, in reading your statement it at least leads me to believe, that there is a possibility that they will not so otherwise terminate operations June of 1982 rather than July 1982 and begin dismantling the plant. That is on page 10.

Mr. Mares. That is if we are unsuccessful in reaching an understanding concerning the continuation of the operations at that location.

Mr. Volkmer. At the present time, do you foresee any commercial plant being constructed in the near future using either the donor solvent or the H-coal liquefaction process?

SFC SUPPORT FOR H-COAL PROCESS

Mr. Mares. The H-coal process is one of the semifinalists in the SFC submission. SFC will decide some time in March as to whether or not this will be one of the projects that they plan to support financially.

Mr. Volkmer. You do not know for sure whether they will or not? In a short period of time, about a week?

Mr. Mares. That was the schedule that has been reported. I do not know whether that is the final schedule for decisions to be made by the SFC.

Mr. Volkmer. Other than that you know of none, right?

Mr. Mares. I know of no commercial plant being proposed using the Exxon donor solvent process and that is the only one I am aware of regarding H-coal.

Mr. **Volkmer**. All right. Well, what concerns some of us is that the glut that we presently have with oils will probably be short-lived in the lifetime of this country at least, whether it will last another 2 or 3 years it is a short span and I am afraid we may see the emphasis on this type of incentive at least we think we started for a while anyway.

H-COAL AND EDS PILOT PLANT

Mr. **Mares**. Well, I think the country can take a great deal of pride in the work of both the H-coal pilot plant and the Exxon donor solvent pilot plant. Those pilot plants have established the technical potential for those two processes. Their value has been proven. The issue at this stage is not whether it is technically feasible or whether the technology has been developed to the stage where a commercial plant can be built, the issue is centered around the economics of a commercial-scale plant. Both those pilot plants were built at a level that would allow commercial-scale judgments to be made. We have achieved that goal so it is no longer appropriate for this Federal Government to continue to fund pilot plant activities at that scale.

Mr. **Volkmer**. On the SRC–1, you've got $129.6 million deferred, still available?

SRC–1 DEFERRED

Mr. **Mares**. There was, at the beginning of fiscal 1982. That significant part of those funds have been spent during fiscal 1982 in accordance with the directions to do the technology baseline that we are going to be providing to the Congress on the first viewing. That has slipped a month; we had originally hoped to have it to you on the 1st of May. It will include both the technology base line as well as the evaluations of that technology. Upon the completion of the baseline we will make a recommendation to Congress as to what should be done with the funds that were not expended in getting us to that stage.

Mr. **Volkmer**. About how much funds will not be expended?

Mr. **Mares**. About $70 million to $75 million.

Mr. **Volkmer**. Thank you, very much. That will conclude my questions for the time being. I may have some later.

ENERGY TECHNOLOGY CENTERS

Mr. **Fuqua**. Mr. Mares, what is proposed to happen to the various fossil labs like Bartlesville and the one in Pittsburgh and some of the other laboratories?

Mr. **Mares**. First of all, there has been no final decision regarding any laboratories.

The budget proposal for fiscal year 1983 contemplates a staffing for fossil energy in the field of 411 persons. Staffing today is about 840 at five locations. The budget request for fiscal year 1983 is for $107 million versus a current authorization in fiscal year 1982 of $415 million, excluding the SRC–1 project.

When I evaluated the situations at the five laboratories in the context of the program that we are going to have as well as in the context of a desire to continue what I think has been a very sound split between the amount of research that is done in the ETC's versus that which is done in industry, I found that over half of our research in the Office of Fossil Energy has been done by industrial laboratories over the last several years. In fact, substantially over half of it. The judgment I have made, taking into account that wherever we have a laboratory activity we should seek to achieve a critical mass of people and skills; so that we are not too small to be a long-term, viable operation. Given that, I have made the tentative recommendation that we should seriously consider terminating activities at the three smaller laboratories.

Mr. **Fuqua**. That would be?

Mr. **Mares**. Those laboratories are from the smallest to the largest: Grand Forks, N. Dak.; Laramie, Wyo.; and Bartlesville, Okla.

Mr. **Fuqua**. You have recommended then that they be terminated?

Mr. **Mares**. Not precisely. What I recommended was that we should try to find another institution which had a potential scope and horizon of activity into which this current facility could be transferred, using that base for the short term, activities that would continue to be done for the Office of Fossil Energy. This new institution would then be able to take the facilities with most of the people, and embark upon a wider range of energy research. It would not just for the Office of Fossil Energy, but potentially for other Government agencies and the private sector and, thereby, increasing and expanding the scope of operations at those locations beyond that which we could foresee in the course of the 1983 budget. In that connection we have reviewed with each of the congressional delegations the possibility that this would be one of the opportunities or possibilities for those centers. In the case of Grand Forks and Laramie, those delegations suggested that we review with the universities at those two locations that are contiguous with the laboratories, the possibility of transferring the lab to them. We have had those discussions. In addition we have also had discussions with regard to Bartlesville with about five for-profit entities and about four not-for-profit entities who would like to learn more about the opportunity to help create a new institutional framework for activity at that location that would conduct work not only for the Federal Government, Office of Fossil Energy but also for other agencies as well.

Mr. **Fuqua**. Have you got a response back from the universities, or those that you have proposed, maybe the operator?

Mr. **Mares**. Yes, sir. I have talked recently with the President of the University of North Dakota, and we have a joint study underway with the University of North Dakota and DOE. I think there is a great deal of interest there. There has been cooperation in the past. I think it complements both their academic pursuits as well as their research pursuits.

I have also talked with the acting president of the University of Wyoming along the same lines and I expect that they will submit a proposal to us as well.

There have been initial conversations, as I said, with several not-for-profit and for-profit organizations relative to Bartlesville. One is visting Bartlesville today. I will talk with one tomorrow. I have also talked with one this week. There seems to be a genuine interest in helping to create in Bartlesville, a new institution which would conduct petroleum-based research not only for the Federal Government but also for other agencies and build upon the skills that are there.

Mr. **Fuqua**. Would you subsequently fund them through grants?

Mr. **Mares**. What I would contemplate is that we would initially agree on a contract for a period of 3 years for the lab to perform work at the level that I would have been able to fund under 1983 budget proposal. I do not have adequate funds in the budget proposal for fiscal year 1983 to continue to fund the laboratory at the level that it has been funded at the past. We do want to continue to do some work in oil shale, enhanced oil recovery and low-rank coals; oil shale at Laramie, enhanced recovery at Bartlesville and low rank coals at Grand Forks. I would contemplate entering into a contract to have work done at Bartlesville, and would also expect that the new institution would market its capabilities and find other entities that quality research done at that location.

Mr. **Fuqua**. Mr. Gore?

Mr. **Gore**. Thank you very much, Mr. Chairman, in letting me come in and ask questions and get back out again.

MHD

Mr. Mares, I have got a real problem with what your Department is doing with magnetohydrodynamics. I have made you aware of that. I think that the Department is behaving in a way on this project that is actually contemptuous of its obligation to work with the legislative branch of Government. DOE is proceeding with termination in spite of the fact that you have not submitted a deferral request. You told Congress over and over and over again that a deferral was coming, but none has come. First you said February 8 and then you said throughout the month of February that one was coming eminently and you yourself said March 15. It is now past that date. You are terminating the project without using the proper procedure and you are doing so in spite of the expressed intent of the Congress. I think it is a highhanded action that violates the relationship that we ought to have between the two branches of Government. Beyond that, you have the disingenuous allegation that MHD is too expensive because of the high cost of a pilot plant but this has never been a prerequisite or part of MHD work and is not nor has ever been proposed for this current budget year or any previous budget year. MHD research stands on its own year by year.

Next, DOE says that private industry will pick up the work but absolutely no effort has been made to check that contention out with industry. There has been no effort to develop a transition. Millions of dollars are going to be lost if there is a complete shutdown and a terrific loss for the taxpayers. The Department has chosen an impossible date for congressional action and seems to care less about the role of Congress in this matter.

Last year, June 1, was chosen and imminent congressional action by that date brought a concession from the administration. This year, I was told that only an absolute and complete action would prevent a shutdown regardless of the intent of the Congress.

Why have you not sent a deferral to the Congress, Mr. Mares?

Mr. **Mares**. The deferral has been signed and is on its way. It was signed today.

Mr. **Gore**. It was signed today? And when is it going to be sent here?

MHD DEFERRAL AND TERMINATION

Mr. **Mares**. I assume forthwith. The deferral was identical to everything that was in the budget of February 8. There are no surprises, with one exception, the deferral states, as its effective date for effectiveness, April 1. At the hearing last week with the Secretary before Congressman Murtha, we said we would not initiate any termination actions before the 15th of April, recognizing that we expected the deferral package to be sent to the Congress this week. The formal package says, "April 1st". We chose not to make an effort to mechanically change it for fear it might have delayed the formal submission. Regardless, there are no termination actions underway for MHD.

We have, I think very prudently, advised the contractors with whom we are dealing that there was a possibility because of our budget, we would initiate termination actions on the 1st of April. That was to alert them that if we, in fact, did it they would have forewarning and we would not just walk to them on March 31 or April 1 and say, "it is cut off." We alerted them to this possibility. But, my understanding is that we have not formally initiated any termination actions.

Mr. **Gore**. It seems to me a fair conclusion that the April date was chosen in an effort to try to ramrod this through with the least possible time for any action by the Congress. Do you agree with that?

Mr. **Mares**. I do not agree with that whatsoever. That date was chosen in February 8's budget. It has been publicly known since February 8.

Mr. **Gore**. It was chosen February 8 for that reason?

Mr. **Mares**. No, sir. In the budget that was submitted to Congress on the 8th of February it was contemplated that there would be a deferral package during February which would have allowed Congress time to decide about the deferral package before the 1st of April. Since the deferral package was not submitted as soon as we anticipated, we reviewed the schedule, looked at our program again to determine if we could in good conscience defer the date from a practical standpoint. We have done that and we will not initiate any terminations before April 15.

Mr. **Gore**. But the date, as it was originally chosen was intended to prevent the Congress——

Mr. **Mares**. I do not believe that one could draw that conclusion from that date at all, Congressman.

Mr. **Gore**. Well, the June 1 date as last year would have been far more appropriate.

Mr. Edwards went to great lengths to avoid the word, "reprograming" in his appearance before the chairman's subcommittee.

Does this mean that the Department is going to spend the money for termination which is absolutely not what it was appropriated for?

MHD TERMINATION

Mr. **Mares**. The Department's request for fiscal year 1983 is for zero funds for MHD. We are, in addition, transmitting a deferral request which includes deferral of some funds from MHD, in the contemplation of initiating a termination.

Mr. **Gore**. Well, where is the money for the termination coming from?

Mr. **Mares**. We are not requesting funds in fiscal year 1983 for MHD. This is the President's proposal for fiscal year 1983. It is only prudent to spend funds in fiscal year 1982 to scope down the program if we are going to terminate the program.

Mr. **Gore**. Well, it is only prudent to spend money that has been authorized and appropriated by the Congress. But, where is the money going to come from? Is it going to come from the Christmas fund? Is it going to come from some other technology?

Mr. **Mares**. In the event that Congress does not agree with deferral it is quite clear that we cannot terminate the program.

Mr. **Gore**. Are you using the pilot project as a justification for trying to kill this program?

Mr. **Mares**. We have several different technologies in the Office of Fossil Energy that are designed to improve the technological base for this country in making electricity, both more efficiently and more environmentally acceptable. They include fuel cells, pressurized-fluidized bed, and the integrated gasification combined cycle. We are continuing in those areas which are going to improve our situation. When I examine the costs to develop the technology in MHD that cost is on the order of $500 million. When I compare that cost versus what I have as resources to spend to provide other options to develop technologies that will make electricity, I do not think that is a wise expenditure of the funds.

Mr. **Gore**. Well, we of course disagree on that and that is not in this budget year. You said there was no termination underway. Have the contractors received their money?

Mr. **Mares**. The contractors are receiving their funds on a monthly basis in accordance with applicable contract documents.

Mr. **Gore**. On March 5, the director of the project wrote you a letter saying, "why has the funding been cut off " and there has been no reply to that letter. The contractors have not received the money. They have been notified that it is going to be terminated. The Congress is sitting here in good faith and trying to work with you and you are proceeding on your own as if the Congress is not a partner in this Government.

Mr. **Mares**. I do not believe that is so, sir.

Mr. **Gore**. Have you replied to that letter from the project?

Mr. **Mares**. This is a letter that I saw an hour ago, dated March 5, from a Dr. Wu at the University of Tennessee, to the Chicago Operations Office. The letter was a response to the Operations Office's standards contracting officer's note alerting the contractor not to expend contract resources in excess of those already in his possession. The note did not indicate that the contract was being terminated, nor that further increments of funding would not be forthcoming. The Operations Office has not terminated that contract. We reviewed, briefly, before I left the Office to inquire of the Operations Office the status of their response to that letter. The letter was addressed to the Operations Office, not to me. A copy was provided to my staff here, and I have just looked at it.

My understanding is that the DOE Chicago Operations Office has reviewed that subject with the university and they are aware of the exact status of the funding and they are working it out.

Mr. **GORE**. Did they review it with them within the last couple of hours?

Mr. **MARES**. I do not know when the conversation took place. I know that at the time that we called them at the Operations Office they had already talked to the university. I do not know when that conversation took place.

Mr. **GORE**. Well, I have gone over my time, Mr. Chairman.

Mr. **VOLKMER**. Will the gentleman yield? I would just like to ask Mr. Mares a little question on MHD.

You mention that you are looking at an investment of $500 million down the road.

COST OF "PROOF OF CONCEPT" FOR MHD

Mr. **MARES**. To get through the proof of concept effort, yes.

Mr. **VOLKMER**. Proof of concept, OK. What time period are we talking about, do you vision spending that $500 million? 5 years, 6 years?

Mr. **MARES**. Probably over a longer period. Probably at least 3 to 4 years of technology development effort before one could decide to proceed with such a proof of concept. Probably 4 years at least from the time of decision to do a proof of concept before it would be up and started. There is then a period of time of operation before information and data becomes available that the private sector or others could use to decide whether or not to try to commercialize the effort.

Mr. **VOLKMER**. Well, it is a concept that is dissimilar from fluidized beds or fuel cells or liquefaction or anything else, is it not?

Mr. **MARES**. Quite dissimilar in its technology but its net result of electricity is identical.

Mr. **GORE**. The net result of pollution and efficiency is superior.

Mr. **MARES**. The other advanced technologies that I previously mentioned also promise considerably more improvement in efficiency and economic and environmental capabilities than the technologies that are used today. We are only working on things that are better than what we use today.

Mr. **GORE**. And you are stopping most of them?

Mr. **MARES**. No, sir, we are not. We are continuing proof of concept in PFB and in fuel cells.

Mr. **GORE**. Thank you, Mr. Chairman.

Mr. **FUQUA**. Thank you, Mr. Gore.

Mr. Skeen?

Mr. **SKEEN**. I have no questions.

Mr. **FUQUA**. Mr. Sensenbrenner?

Mr. **Sensenbrenner**. I have one very brief question.

It is my understanding that in the MHD program most of the research has been completed and the next step is a demonstration project. Am I correct in this impression?

MHD CONTINUING DEVELOPMENT

Mr. **Mares**. No, sir. I do not think that is right. My understanding is that there must be a continuing technology development effort that might cost as much as $150 million after which a decision would be reached as to whether to build a proof of concept scale facility of about 200 megawatts electrical output, which could require another $350 million in overnight construction costs. That is over the time scale I just indicated to Mr. Volkmer.

Mr. **Sensenbrenner**. If your research has not been completed, how much money has been spent on research on MHD up to date?

Mr. **Mares**. There has probably been about $400 million spent on this technology to date.

Mr. **Sensenbrenner**. In other words, if Congress does accept the President's recommendation, the $400 million, in effect, goes down the drain because we still have $150 million worth of research to go before we reach the demonstration project stage?

Mr. **Mares**. No, sir. I do not believe it goes down the drain.

Mr. **Sensenbrenner**. Then what happens to it?

Mr. **Mares**. Well, the same thing that happens with most technologies; the R. & D. that has been conducted to date and has been funded by the Federal Government becomes available to the public through published reports, that becomes what one might call the technology base of the country, and it may be used by any firm or individual that has a use for it.

We have underway this year, two different efforts logically to package and evaluate the technology that has been developed to date so that there will be a good baseline from which the Nation—the private sector—at any stage could decide to continue to develop this technology further. So, we are identifying where we are today, what has not been done, and what should be done if there was a decision at some future date that it was important, necessary, or desirable to continue to develop the technology.

Mr. **Sensenbrenner**. Do you consider this a high-risk project?

Mr. **Mares**. Yes, sir. It is a high-risk project.

Mr. **Sensenbrenner**. Do you consider it a long-term project based on your testimony?

Mr. **Mares**. Yes, sir. It is a long-term project.

Mr. **Sensenbrenner**. And how does zeroing this out fit with the announced determination by the administration to fund high-risk, long-term projects such as you have started out in your prepared testimony by supporting?

Mr. **Mares**. It is very easy; it is not possible to support every long-range, high-risk project. We have selected those projects that we think are going to be of the most value for the dollar. The ones that we have selected yield improved efficiencies, and also improve the environmental aspects related to the generation of electricity. Fuel cells, PFB, and the integrated combined gasification projects are long-range, high-risk activities that we have underway as well.

Mr. **Sensenbrenner**. Well, I must say that I am getting more confused by your testimony as time goes on. We hear from representatives of the administration that we have sunk so much money into the Clinch River breeder reactor that we ought to keep on funding that, whereas, on this particular project, apparently $400 million is not a sufficient investment for us to continue funding it. I guess that I am one of the members of this subcommittee and the full committee that has a record of being more sympathetic with the President's budget request than some of the other members of both the subcommittee and the full committee. But, believe me, I am going to need to have a lot better explanation than what I have heard today from you in order to support the policy decisions that have apparently been made by the Department of Energy and by the Office of Management and Budget. I am serving notice on you that you are going to have to give us a lot better ammunition to defend what you are going to do than I have been hearing from you and others today.

Thank you very much.

Mr. **Volkmer**. Will the gentleman yield?

Mr. **Sensenbrenner**. I yield.

Mr. **Volkmer**. I would just like to comment too that when we talk about MHD, that is another way to generate electricity, so is nuclear another way to generate electricity.

Just following on from the comments of the gentleman from Wisconsin, when we see all this money going over on this end, we are not talking about nearly the same amount of money on MHD are we?

I know that you are looking at your area and yours is fossil fuel but we are looking at the whole area and you will have to understand our problems, I hope.

Mr. **Mares**. I happen to think that the balance we have proposed in the President's budget is sufficient to develop the various fossil fuel technology in this country and is responsive to the needs of the country.

Mr. **Volkmer**. Well, it is not the total amount—will the gentleman yield further?

Mr. **Sensenbrenner**. Yes.

Mr. **Volkmer**. It is not the total amount that you really started out with when you went to OMB is it, originally? Not near that—you wanted around $400 million did you not?

Mr. **Mares**. The Department submitted to OMB a request of $305 million for fossil energy but that budget did not include anything for MHD either, sir.

Mr. **Sensenbrenner**. I yield back the balance of my time.

Mr. **Fuqua**. Thank you, Mr. Sensenbrenner.

Ms. Schneider?

Ms. **Schneider**. Thank you, Mr. Chairman.

In your testimony you state that various industries have historically—that have been involved in coal—will continue to expand the role of marketing and developing of new fossil fuel technologies. Some of those industries have been, for example, the extraction companies, the mining companies, the railroads insofar as the transportation is concerned, and then utilities which relate directly to the consumers.

I am curious to know which of those industries you believe will expand the use of coal in developing and marketing in the new fossil fuel technology?

EXPANSION OF THE USE OF COAL

Mr. **Mares**. The same industries that have been using coal all along—the ones that you have mentioned.

Ms. **Schneider**. Well, I wonder which of those do you feel have the motivations to do that in the future?

Mr. **Mares**. They all do.

Ms. **Schneider**. Given the current economy?

Mr. **Mares**. Given the current economy.

Ms. **Schneider**. And to what degree do you think they will be expanding the use of coal? For example, let us just take the mining companies themselves.

Mr. **Mares**. The mining companies mine it. They do not use it themselves generally, unless they have powerplants on their site.

Ms. **Schneider**. Granted, but I am referring to the marketing of new technologies. Do you feel that with our current oil glut that there is going to be enough of a demand on fossil fuel companies to get involved in new technologies?

Mr. **Mares**. I think that the fossil fuel industry is one of the most vibrant industries in this country. It is made up of very competent manufacturing companies as well as very competent consumers of fossil energy. I have the utmost confidence in their ability and wisdom to develop and use commercialized technologies which are going to allow them to use the lowest-cost fuel that exists in the United States; namely, coal.

Ms. **Schneider**. Well, let us just take the railroad industry for a moment. I wonder if you could give some specifics as to their investment or involvement in new coal technologies? Some specifics that we can have for the record.

Mr. **Mares**. In what sense?

Ms. **Schneider**. Just to improving the varieties and opportunities for coal technologies. Have they done anything?

Mr. **Mares**. The railroads today transport the coal for the United States. Generally their motive power is diesel power. The people that are the prime users of coal are those that combust coal for power or for processed heat.

Ms. **Schneider**. But we do not see any investment from their budgets in research and development.

Mr. **Mares**. I disagree with you. There is an enormous amount of private-sector money that is spent in developing ways to utilize coal.

Ms. **Schneider**. But you are not giving me any specifics.

Mr. **Mares**. We have a large number of cofunded projects where we, as well as various industrial firms, are spending money on developing technologies to utilize coal. This is in the area of various types of combustion, coal-water mixture, coal-oil mixtures, and coal liquefaction and coal gasification.

Ms. **Schneider**. OK. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you.

Mr. Walker?

Mr. **Walker**. No questions.

Mr. **Fuqua**. Did you have a question, Mr. Volkmer?

Mr. **Volkmer**. Yes. I would like to get to the pressurized-fluidized bed part, if we may.

According to the notes I have, last year I guess it was, about $15 million was requested for the Wood Ridge PFB operation's facility and about $700,000 to close out our contribution to the Grimethorpe operation in England, right?

Now, this year, we are terminating Woodridge and we want to fund Grimethorpe at $2.5 million; is that a little inconsistent? Which way are we going?

GRIMETHORPE PFB

Mr. **Mares**. No, sir. I do not think it is at all inconsistent. Grimethorpe is a pilot plant to test the concept of the PFB combustor and has been funded by the United States, Germany, and the United Kingdom. It is an international agreement. That project is now completed and has been operating already and providing data for the three parties that are participating.

The proposal for fiscal year 1983 is to conclude our participation in that project and complete our international obligation.

Mr. **Volkmer**. And that $2.5 million is all that is necessary to do that?

Mr. **Mares**. That is our judgment. Earlier estimates of the cost to complete the last phase of that initial commitment were that it would have been more than $2.5 million, but as we have gotten closer to the detailed scheduling of that last phase of the work it is our judgment that when the participants look at it carefully, they will conclude that the $82.5 million level of funding from each of us will be sufficient to successfully conclude the important work that was planned to be done by that project and, thereby establish the concept——

Mr. **Volkmer**. When do they plan to have testing completed?

Mr. **Mares**. We would expect that fiscal year 1983 would be our last year of funding——

Mr. **Volkmer**. That is your last year of participation?

Mr. **Mares**. Yes, sir.

Mr. **Volkmer**. And you estimate then that we do not need Wood Ridge any more then?

Mr. **Mares**. The judgment was that we could not justify continuing two proof-of-concept efforts in the PFB combustion area. Albeit, there has been a significant amount of Federal expenditure to date on the Wood Ridge facility. We have sought, since last fall, to discuss with Curtiss Wright, the possibility that instead of our dismantling or entering into negotiations for termination that instead the termination costs be applied toward completing the project that Curtiss Wright, coupled with those other private-sector firms that they were able to encourage to participate, would then come up with the balance of the funds needed for construction of that plant and subsequent operation.

The operating program for that plant is expected to be about $6 million per year for 2 years. About $16 million is needed to complete that plant and as of this moment we do not have a firm commitment as to either the funding for either the operation nor the completion of the plant. There are some expressions of interest from people that would be interested in participating in a significant portion of the operating costs. As of this moment we do not have anybody that has indicated—including Curtiss Wright that they would participate in a major way in the completion of that project.

PFB COMBINED CYCLE PLANT DEFERRAL

Mr. **Volkmer**. Is that included in your deferral?

Mr. **Mares**. Yes, sir.

Mr. **Volkmer**. For this year?

Mr. **Mares**. Yes, sir.

Mr. **Volkmer**. As of June, or April?

Mr. **Mares**. It is one of those that we would not initiate formal termination before April 15.

Mr. **Volkmer**. And how much funds would be deferred?

Mr. **Mares**. $6 million.

Mr. **Volkmer**. And what do you plan to use that money for then?

Mr. **Mares**. That is part of the overall restructuring of funds that we are recommending be used for the consolidation of our field structure that includes termination of activities at the centers, separation of personnel and costs entailed therein.

Mr. **Volkmer**. How much do you envision it will cost to terminate that contract at Wood Ridge?

Mr. **Mares**. Our judgment is that we have no legal obligation in the termination of that contract. There is a difference of opinion. Curtiss Wright thinks there is a substantial obligation. Around $10 million.

Mr. **Volkmer**. Thank you.

Mr. **Fuqua**. Thank you, Mr. Volkmer.

Mr. Mares, in response to a question asked by Ms. Schneider, although you talked about the cofunding with some regulated industries, last fall, I think it was, had a number of utilities come in and testify that based on the present rate structure, their inability to initiate new technologies and develop new technologies because the rate structure had lagged so far behind actual cost and return on investment that they were strapped in trying to just keep their heads above water at this time. Now, it makes it difficult for me to understand the rationale that you have in trying to terminate some of those joint projects that you have to develop new technologies. I assume you subscribe to the theory that they are going to pick them up, is that correct?

Mr. **Mares**. In many cases we are optimistic that the projects will be considered to be quite important and will be picked up.

The utilities have been supportive of several projects; in both individually and in the context of the EPRI organization.

Mr. **Fuqua**. And GRI.

Mr. **Mares**. GRI which is the counterpart.

Mr. **Fuqua**. Right.

Mr. **Mares**. In addition to the utilities that are involved with the development of technologies there are a significant number of other manufacturing firms that are manufacturing equipment is used by the utilities to generate electricity. They have been involved in projects with us sometimes with the utilities and sometimes by themselves. In several cases, I would say that we are optimistic that they will proceed with further development of the technology. In the high temperature turbine program we have been working to develop the concepts that would allow temperatures of 2,600° F to 3,000° F to be used. This is a substantial improvement over the current levels of around 1,900° to 2,000° F for combustion turbine capabilities. We are optimistic that this proof of concept work be completed.

Mr. **Fuqua**. Did they tell you that? Did those people that are developing that technology tell you that?

Mr. **Mares**. My understanding is that they are optimistic that they will be able to develop that concept. Yes, sir.

Mr. **Fuqua**. Well, they have either testified or had a meeting with Members of Congress and I can name names if you want me to, who said there is no way that they could continue that. They were pleading with us to try to restore those funds.

Mr. **Mares**. That is correct. They have certainly testified that they believe that the Federal Government should continue with a technology base program in the heat high temperature turbine areas.

I am also aware that at least one firm is trying to develop and was rather optimistic that they could develop, a testing vehicle that would carry the particular concept of high temperature turbine on to the next step.

Mr. **Fuqua**. Well, they have repeatedly—and I have asked every one that has testified if they felt that their own company had the funds. They said anything that was longer than 5 to 8 years for commercialization before it was ready for commercialization, that there is no way they can go to their stockholders and get those funds to support that type of research. They were willing to put company funds in it on some type of joint program with the DOE, but to take that risk themselves, they would not be able to do that. Here again, as Mr. Sensenbrenner was telling you earlier, we have put quite a bit of money into developing the basic technology and we are really into the testing program now—a further refinement of the development stage and we are pulling out.

Mr. **Mares**. I think that based on what I understand, there is a possibility that there will be a joint effort to take one of the concepts of high temperature turbine technology to the next stage of development.

It is also clear that from the conversations that I have had, that the firms would prefer and would like to have the Federal Government continue to fund technology base development. I think in this particular area, as well as in most of the areas, until there is no Federal funding none of us are going to know for sure what the industry will do.

Mr. **Fuqua**. The one that you were talking about that may be funded by industry, is that water-cooled——

Mr. **Mares**. I know the firm but I do not remember which technology. There are two technologies and two firms.

Mr. **Fuqua**. If you can tell me the firm, I can tell you which one it is.

Mr. MARES. General Electric.

Mr. FUQUA. They are still pleading for funds.

Mr. MARES. I am aware of that.

Mr. FUQUA. I have talked with them and they talked with me about that.

Mr. MARES. I am aware of that.

Mr. FUQUA. In the deferrals that you are sending up, should Congress disapprove those deferrals, what or how is your manpower affected?

Mr. MARES. Manpower will not be affected by the disapproval of those deferrals.

Mr. FUQUA. What are your current plans for reduction in force?

Mr. MARES. We have a staffing expectation in our budget for fiscal year 1983 of 511 people. Our onboard strength today is approximately 1,090. The expectation is that we would reduce the field to 411 and the headquarters to 100 and that it would be part of the consolidation of ETC's that I described to you earlier.

We have been looking at our organization and our staffing since last summer, long before we had our final manpower or dollar mark for fiscal year 1983. We had concluded, at that time, that there were moves that we could and should make to improve our overall efficiency and effectiveness which would allow us to reduce our overall personnel.

Mr. FUQUA. So, they would be reduced about one-half?

Mr. MARES. Approximately.

Mr. FUQUA. Well, what is your manpower level plan for October of this year?

Mr. MARES. The FTE number for fiscal year 1983 is 511.

Mr. FUQUA. Would that be at the beginning of October 1?

Mr. MARES. Well, the FTE concept is in effect your full-time equivalent personnel in man-years, so that if you are over your ceiling on October 1 for 6 months then you have got to be under it by the same number for the remaining 6 months.

In the Office of Fossil Energy we are currently over our FTE number for fiscal year 1982, but not nearly as much as we are out of line relative to 1983.

Mr. FUQUA. So, you are losing around 600 people?

Mr. MARES. From our current staffing——

Mr. FUQUA. Between now and October? Have any of those notices gone out yet?

Mr. MARES. No, sir. It is not appropriate to send out such notices.

Mr. FUQUA. Not appropriate?

Mr. MARES. The number 511 is a fiscal year 1983 number, it is not a fiscal year 1982 number.

Mr. FUQUA. But you will be in fiscal 1983 on October 1.

Mr. MARES. October 1.

Mr. FUQUA. And to achieve that by that time or on that date?

Mr. MARES. We would plan to take action in early 1983.

Mr. FUQUA. In calendar year 1983?

Mr. MARES. No, in fiscal 1983.

Mr. FUQUA. So, between—probably in the first quarter of fiscal year 1983?

Mr. **Mares**. We would certainly look at making our moves in early 1983, consistent with fiscal 1983, consistent with the budget proposal that we have made.

Mr. **Fuqua**. You indicated that fiscal year 1983 funds are going to be available for the Wilsonville project in 1983. Will the funds come from deferrals or reprograming?

Mr. **Mares**. It is available from appropriations in fiscal year 1982 for the SRC-1 pilot plant effort.

Mr. **Fuqua**. And how much will that be? Approximately?

Mr. **Mares**. It normally costs about $750,000 per month and I gather that there will be about $3 million from EPRI in fiscal year 1983 in addition to what we have remaining from fiscal year 1982. It is not solely a DOE operation. It is owned by EPRI. Both EPRI and DOE are conducting work at the plant.

Mr. **Fuqua**. Why were these funds not expended in 1982?

Mr. **Mares**. My understanding is that the plant has been operating in 1982 in support, to the extent that is necessary, of the SRC-1 project as well as other projects that we have within our coal liquefaction program.

I do not believe that the plant has just been shut down and left idle.

Mr. **Fuqua**. You said that the money was coming from the SRC-1 plant?

Mr. **Mares**. We have an SRC-1 pilot plant in the budget. Not all of that money will be required to be spent in fiscal year 1982 so it will be carried over and spent in fiscal year 1983 in support of the coal liquefaction program.

Ms. **Schneider**. Will the chairman yield a moment on that?

Mr. **Fuqua**. Just one question. You have the base line for SRC-1 demo plan and it is about $129.6 million, I think that is a ballpark figure. How much of that will remain?

Mr. **Mares**. We expected that about $70 million of that will remain in June at the time that we deliver to the Congress the base line estimate.

Mr. **Fuqua**. Ms. Schneider?

Ms. **Schneider**. I am trying to figure out how it was, if I recall correctly. We, the committee, defeated the funds for SRC-1 and then they were put back in and the language of the appropriation's committee, is that correct? And then, do I understand that that money now is being taken by the Department of Energy and can be deferred?

Mr. **Fuqua**. I think that Mr. Mares would be the appropriate one to answer that.

Mr. **Mares**. I am going to complete the base line estimate as directed by Congress. That will be delivered to Congress on the 1st of June at which time we will then make a recommendation to Congress regarding the disposition of the remainder of funds that were not expended.

Ms. **Schneider**. How much do you think that will be?

Mr. **Mares**. I think it will be about $70 million.

Ms. **Schneider**. OK. Thank you, Mr. Chairman.

Mr. **Fuqua**. You mentioned the 25-percent research and development tax credit. Is it not true that that credit is incremental and applies only to current year research and development investments greater than the previous year? It only applies to manpower and not to capital equipment?

Mr. **Mares**. I do not remember the point on capital equipment. I thought it did apply to equipment but I take your judgment on that.

Mr. **Fuqua**. I think it only applies to man-hours.

Is it your intention in the fuel cells to pursue this program to commercial demonstration and to use the fuel cells for central station power generation as mandated by the Federal Non-Nuclear Energy Act of 1974?

Mr. **Mares**. We certainly plan to complete the 4.8 megawatt Con-Edison demonstration facility in New York City. There are funds in our budget for that demonstration.

Mr. **Fuqua**. This has been mentioned, but I wanted to ask again about it. It is my understanding that when the Department of Energy budget went to the White House for your Department, your Secretary of Fossil Energy, it was around $305 million?

Mr. **Mares**. That is correct.

Mr. **Fuqua**. And it is reduced to $107 million.

Mr. **Mares**. That is correct.

Mr. **Fuqua**. Secretary Edwards made a gallant fight at the White House to try and restore that $200 million that was reduced. He did make appeals to that.

Mr. **Mares**. The Department did appeal, yes, sir.

Mr. **Fuqua**. If the wisdom of the committee, to try to restore some of those funds back to that, what I think is a more respectable level, realizing the fact that we still have not solved our energy problems, do you think that the Department then would be able to allocate those funds in some areas of fossil energy as somewhat related to the requests that were made of OMB in that area? I realize that you support OMB and you are a team player; that has been very evident here this afternoon. You should get an "A" for that.

Mr. **Mares**. If the Congress decides that they will increase the budget for fossil energy I would be prepared to review with you or members of your committee and your staff, the technical merits of particular ideas that you would like to consider.

I do not think that it is necessary to increase our budget. I think we are meeting the key fossil energy technology needs for the——

Mr. **Fuqua**. We have already given you an "A" now.

Mr. Nelson, do you have any questions.

Mr. **Nelson**. No.

Mr. **Fuqua**. Mr. Mares, we want to thank you very much. You have done a very good job. They may even put a plaque up down there for you.

Thank you very much for being here and we do look forward to working with you on the fossil budget.

Mr. **Mares**. Thank you very much, sir.

Mr. **Fuqua**. The subcommittee is adjourned.

[Whereupon the subcommittee was adjourned at 3:25 p.m.]

[Questions and answers submitted for the record, plus an additional statement by Richard Wolfe, vice president, Research and Development follows:]

Department of Energy
Washington, D.C. 20585

April 6, 1982

Honorable Don Fuqua
Chairman, Committee on Science
and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

I am writing in response to your letter of March 5, 1982, in which you requested responses to a number of questions related to the FY 1983 Fossil Energy budget request.

The requested written responses are enclosed and hopefully are fully responsive to your questions.

If additional information or clarification is required, please do not hesitate to ask.

Sincerely,

Jan W Mares

Jan W. Mares
Assistant Secretary
for Fossil Energy

Enclosures

cc: Honorable Larry Winn, Jr.
Ranking Minority Member

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Coal Liquefaction

QUESTION: The narrative states that "It is anticipated that an indirect liquefaction industry will be initiated through incentives provided by the SFC." A similar statement is made with regard to direct liquefaction. Suppose a given technology fails to meet the SFC critieria. What implications does this have regarding the advisability of further R&D? Please provide DOE's data that indicates specific increases in private sector R&D in direct and indirect liquefaction.

ANSWER: Liquefaction processes which could expect support from the SFC are those based on commercial or large pre-commercial experience. Individual projects could fail to gain SFC support for any of a variety of reasons not directly related to maturity of the technology. Actually, several liquid fuel projects from the first SFC solicitation have met the maturity criteria; it remains to be seen how they will fare based on business and financial criteria.

The R&D work on liquefaction proposed in the FY 1983 budget relates to advanced process concepts which promise to provide significant improvement in economic or environmental terms over the available technology. This work should be pursued regardless of the outcome of the SFC evaluation of commercial proposals. However, we do not believe that near term applied research on large scale pilot and demonstration projects should again be financed by the government even if the SFC fails to select a coal liquefaction project in the first round solicitation.

The DOE does not have data that relates specifically to future private sector R&D plans. We do know that the non-governmental participants in the EDS pilot plant project plan to operate that plant for several months after the DOE support ends. Also, the

H-Coal participants are seeking support to extend operation of the H-Coal pilot plant. There are a number of other factors that will influence, in a positive manner, the level of private sector R&D activity in the area of synfuel development. The Economic Recovery Act of 1981 contains provisions that will significantly assist the private sector in making decisions regarding energy research and development. The 25 percent tax credit for research and development activities was specifically designed to reduce the after tax cost of energy R&D activity. This should spur private sector development. Also, the regulatory reforms that are underway will remove much of the burden that previously was imposed by the Federal Government on the private sector. In addition, the decontrol of oil prices through the elimination of federal price and allocation controls, has provided increased economic incentives. Past oil price increases has made competing energy technologies, such as synfuels, more attractive. However, private sector activity will continue to depend on future oil price and supply expectations.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Coal Liquefaction

QUESTION:	What proportion of the $4M request for support studies/engineering evaluations will be devoted to environmental and health related research?
ANSWER:	Funds from this budget line support work in environmental and health related research, research guidance studies and engineering assessments, and ancillary processing studies and evaluations. Approximately 30-40 percent of the $4.0M requested for FY 1983 is targeted for support of environmental and health related research.

HS&T Hearing
Energy Development
& Applications Sub
Asked by Don Fuqua
For Hearing 3/18/8

Coal Liquefaction
H-Coal Plant

QUESTION: It is stated that termination and dismantlement will permit a $3M deferral for reprogramming. What is the FY 1982 unobligated balance? What is the dismantlement cost estimate? What is the source and date of the estimate? When will Congress receive the deferral request?

ANSWER: The unobligated balance of funds for the H-Coal plant at the beginning of FY 1982 is $330,364.

Three estimates have been developed by ASFI and three by DOE sources for the shutdown, cleanup and demolition of the pilot plant and range from $10.0M to $34.8M. Five of the estimates were at or below $23.9M. Consequently, $23.9M is considered as the current estimate of dismantlement, the date of which is November 4, 1981.

The Administration transmitted the deferral request to the Congress on March 18, 1982.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Coal Liquefaction

QUESTION: The liquid phase methanol research facility incorporates a number of investigative areas that will be continued in 1983 under other activities, including catalyst development and slurry phase reaction. In view of the multiplicity of research activities available in this one location, what criteria did the agency use in deciding on termination?

ANSWER: Although the areas of research that are being investigated at the liquid phase methanol facility are related to research activity at other locations, the efforts at the facility are more process specific and are in support of the modification to enlarge the scale of testing to the PDU level. The research efforts that are being conducted at other locations, and which wil continue into FY 1983, are of higher priority, longer range and more generic to the technology advancement. Therefore, in view of the fiscal constraint in support of the Economic Recovery Program, it was deemed prudent to eliminate research at the PDU level along with the supporting efforts on that project versus eliminating higher priority activity that is consistent with the rescoped liquefaction program.

HS&T Hearing
Energy Development
& Applications Sub
Asked by Don Fuqua
For Hearing 3/18/8

Mining Research and Development

QUESTION: The narrative states the "FY 1983 budget was prepared on the assumption that a consolidation of mining activities now conducted separately by the Agency and DOI would be implemented during FY 1982. The reorganization proposal to place fossil energy R&D activities under DOC may require re-examination of this assumption." What would the 1983 request have been if the DOI consolidation assumption had not been made? Under DOE reorganization, how will the program re-examination be directed?

ANSWER: If the DOI consolidation assumption had not been made, the DOE FY 1983 budget request for Mining R&D would have been $1,000,000.

The program re-examination has been completed by holding discussions with DOC planners following up the President's Sunset DOE reorganization decisions. DOC and DOE are in full agreement that DOE's Mining R&D program, exclusive of the Coal Preparation activity, should be transferred as soon as possible to DOI/BOM. This is necessary to allow active integration of the mining program with its counterpart mining activities in the BOM in order to effect maximum operating efficiencies in mining research beginning not later than September 30, 1982.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Surface Coal Gasification

QUESTION: For each of the facilities slated for mothballing, provide a cost estimate for the mothballing operation, estimated annual costs for maintenance, security, etc., and the source and date of the estimate.

ANSWER: The attached table lists the Pilot Plant projects slated to be shutdown or completed this year and their estimated mothballing and annual maintenance costs.

The sources of the estimates of costs are varied. It should be noted that the actual costs of both activities will depend on the type/ degree of mothballing initiated as well as the projected possibility of whether or not the facility will be used again. It should also be noted that the mothballing costs indicated do not include any costs associated with the ultimate disposal of the facilities (i.e., dismantlement) which are subject to further negotiations.

SURFACE COAL GASIFICATION

DISPOSAL OF FACILITIES

ESTIMATED MOTHBALLING AND ANNUAL MAINTENANCE COSTS
(Thousands of Dollars)

Project	Mothballing Cost	Annual Maintenance Cost	Source and Date of Estimate	
BIGAS Pilot Plant	3,068	1,447	METC	2/82
Westinghouse Pilot Plant	2,550	310	HQ/METC	2/82
Rockwell Hydrogasification	700	120	METC	2/82
Rockwell Molten Salt	20	70	HQ	3/82
PEATGAS	652	360*	IGT	3/82
U-Gas	156	231*	IGT	3/82
CE Atmospheric Entrained Bed	551	245	CE	11/81
TOTAL	7,697	2,783		

* IGT has offered to accept abandonment in place. These costs could be zero if decision is forthcoming in next 3 months.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Surface Coal Gasification

QUESTION: Concerning the environmental research that will be terminated at Grand Forks and Morgantown, where will this work be continued?

ANSWER: The objective of the current environmental research work which is being conducted in conjunction with the PDU scale gasifier programs at the Morgantown Energy Technology Center (METC) and the Grand Forks Energy Technology Center (GFETC) is to understand the process/effluent relationships between the operational conditions used in these gasifiers and the individual steps of a waste water cleanup system available at these facilities. This objective as related to the performance of these fixed bed systems essentially will be accomplished in 1982. One of the next phases of the environmental research program at the PDU scale is concerned with understanding the problems associated with system integration. It is also concerned with the inter-relationships that exist between the various process steps of an integrated gasifier/cleanup system during operations which simulate the actual end use application of that system in a synthetic fuel plant. One of the principal efforts in this area currently is being conducted at the General Electric gasification research facility in Schenectady, New York. This facility has a unique "closed loop" cleanup system which provides the flexibility to distribute and recycle liquid effluents throughout the entire gasifier/cleanup system. In this manner, research can be conducted on optimum internal recycling of effluent streams so as to maximize overall process efficiency while minimizing the environmental impact of effluents discharged from the plant boundary.

A balanced environmental research program will continue to be conducted at the remaining Energy Technology Centers in FY 1983. This environmental program will consist of experimental (bench scale and slip stream) studies and analytical research. It is organized into four categories: Process/Effluent Relationships, Process Modification for Environmental Control, Effluent Control Technology and Environmental Systems Engineering/Integration.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Surface Coal Gasification

QUESTION: What are the Administrations' objectives for environmental analysis referred to in the narrative?

ANSWER: The objectives of the environmental research and development program for surface gasification are to ensure that:

- Potential health and environmental concerns related to surface gasification are adequately addressed;
- In-process controls on environmental control technologies will be available to resolve these concerns; and
- An adequate environmental data base will exist to assess the health and environmental impacts of a large scale coal gasification industry.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Surface Coal Gasification

QUESTION: What is the request for the Mountain States Energy Gasification Project?

ANSWER: The FY 1983 budget request does not contain any funds for the Mountain Fuel Entrained-Bed Gasification Project. It is anticipated that the funds currently available from DOE and the cost sharing partner will be sufficient to complete the construction of the facility and to prepare for the initiation of the test program. These activities will take place during FY 1982 and FY 1983.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Underground Coal Gasification

QUESTION: What is the extent of commitment for environmental monitoring?

ANSWER: Agreements between the DOE and the Departments of Natural Resources in Wyoming and West Virginia were signed calling for five years of post-burn environmental monitoring following any active underground coal gasification tests. Sites at Pricetown, West Virginia, and Hanna, Hoe Creek, and Rawlins, Wyoming, all are included. In addition, if hydrological monitoring shows that the water on the Wyoming sites is not returning to its pre-burn state, active aquifer restoration is likely to be required. The field tests were completed between 1979 and 1981 so the DOE commitment extends to 1986. Annual cost will decrease as sites are abandoned but about $700,000 per year will be needed through FY 1986. If active aquifer restoration is needed at any site, this funding will be an additional cost.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development

QUESTION: The narrative states that combustion of coal-derived synfuels will not be pursued in FY 1983. How many projects currently supported will be terminated in FY 1983?

ANSWER: Of the seventeen projects currently supported under the synfuel combustion program, eight will be completed with FY 1982 funding as originally scheduled. Of the remaining nine projects, seven will be brought to an orderly completion with FY 1982 funding by reducing the scope of the originally planned effort. The remaining two efforts, which focus primarily on gas phase kinetics and are thus germaine to both synfuel and coal combustion, are being examined for possible continuation.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development

QUESTION:	In justifying termination of research in the combustion of coal-derived synfuels, the Agency refers to the large level of assistance available from the SFC. What specific projects now under SFC consideration or included in Agency programs for FY 1983 are directed toward end-use of coal-derived synfuels, i.e., combustion?
ANSWER:	The decision to terminate R&D on coal-derived synfuel combustion in Fossil Energy and the reference to anticipated SFC activities in this regard is based upon the following considerations. In view of the state of technology development required for SFC support we anticipate that the majority of projects presented to the SFC over the next few years will be based upon gasification and indirect liquefaction technologies. Because of the compatibility of fuels produced by these processes with traditional combustion equipment, no significant technical combustion issues are expected to arise. Technical combustion issues may arise in optimizing the utilization of fuels obtained through direct liquefaction processes. However, fuels from these processes are anticipated to be utilized primarily as specification fuels in automotive and distillate furnace oil applications, and the private sector has available the expertise and knowledge base to address combustion and other related issues that may arise in optimizing the use of these fuels. It is certainly logical to expect that companies willing to invest hundreds of millions of dollars for a synfuel production facility will perform the necessary R&D on combustion and use of the products which are produced in these plants.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development

QUESTION: Considering that SFC supported projects will not have products in the marketplace before 1986, what incentive will users have to perform this kind of applied research in 1983?

ANSWER: We believe that sufficient technical expertise and incentives currently exist in the private sector to ensure that the products of a synfuel facility can be marketed in an economical and environmentally acceptable manner. Should technical issues arise regarding the combustion or other utilization of synthetic fuels, we believe private firms will continue to support the R&D necessary to ensure the marketability of their anticipated product slates, and to assure a return on their large investments. Sufficient competition exists between firms proposing to build synthetic fuel plants to ensure that sufficient incentive exists to conduct this type of R&D.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development
Materials and Components

QUESTION: Which specific projects will be curtailed in scope as a result of the reduced request? Please indicate the performers of M&C research.

ANSWER: Nearly all of the AR&TD materials projects now (FY 1982) underway which will be continued in FY 1983 will be reduced in scope. Projects which will be curtailed due to the reduced funding level include:

Project	Performer
CE-3.1 - Microstructural control to improve properties of weldments in chromium-moyldenum steels	Combustion Engineering
UCON-3.1 - Study relationship of microstructure and fatigue properties of weldment specimens of chromium-molybdenum steels	University of Connecticut
ANL-4.1 - Develop oxide-dispersion strengthened alloys	Argonne National Lab (ANL)
CSM-3.2 - Electroslag welding of 2 1/4 Cr-1 Mo Steel	Colorado School of Mines
ORNL-3.3 - Determine effect of heat treatment on microstructure and mechanical properties of modified low allow steels	Oak Ridge National Lab
WEC-3.3 - Investigate hydrogen embrittlement of chromium-molybdenum alloys	Westinghouse Research Lab
MPC-3.4 - Perform autoclave exposure tests and identify corrosion mechanisms	Metals Properties Council
MPC-4.4 - Evaluate materials subjected to combined corrosion and erosion	Metals Properties Council
ANL-6.4 - Investigate corrosion effects of ash and sulfate deposits	ANL
PITT-6.4 - Investigate the hot corrosion of alloys in gas turbine applications	University of Pittsburgh

BOMT-4.9 - Study alkali attack and CO disintegration of refractories	Bureau of Mines (Tuscaloosa)

Peformers planned for inclusion at the $3.940M level include:

Battelle Columbus Lab
University of California, Berkeley
University of Tennessee
Cornell University
University of California (Santa Barbara)
University of Illinois
Idaho National Engineering Lab
General Electric Company
Massachusetts Institute of Technology
Lawrence Berkeley Labs
Iowa State University
Virginia Polytechnic Institute
Pennsylvania State University
Los Alamos National Lab
National Bureau of Standards
ANL
ORNL

The AR&TD components program at the reduced FY 1983 level of $0.860M will curtail the following:

Project	Performer
Slurry Pump Development	Rockwell International
Slurry Let Down Device Development	Jet Propulsion Lab

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development
Program Development and Coordination

QUESTION: Noting that the Agency is planning to reduce the number of Energy Technology Centers (ETCs), what criteria has been used in deciding which Centers will close? When will closing occur? How many Federal employees will be affected? How many contract employees? Please provide the estimated costs of separating these employees, by ETC.

ANSWER: No final decisions have been made as to the future shape of the entire Fossil Energy structure. Recommendations on the total structure (Headquarters and field) are being prepared by the Assistant Secretary for Fossil Energy (ASFE) for review by the Secretary. Prior to submissions of the FY 1983 budget, studies indicated that restructuring of field centers and defederalization of allowable activities could improve the efficiency and effectiveness of the Fossil Energy Program.

Annual operating costs for the Centers, including research, today is about $70 million. Expenditures of this amount under the FY 1983 budget would leave less than $30 million for research outside the ETCs. The FY 1983 budget, including program content and staffing levels , therefore necessitate the restructuring and consolidation of the entire organization.

We are presently examining various options to realign and streamline the field structure to maximize the use of the limited federal personnel, equipment and expertise to execute the program; both in the transitional period in FY 1982 and the final organization in FY 1983. Critical factors in determining the final organization and structure include the size and character of program reductions, judgment as to the critical staff capabilities, interest in outside entities for possible assuming of Center responsibility and liability, and program content to be conducted at each Center for which capability and expertise exists.

The ASFE has had considerable discussions with the members of Congress and their staff as well as other government agencies, university groups, research groups, and private sector (both profit and non-profit) organizations relative to the future of BETC, LETC, and GFETC. The ASFE is developing proposals for these Centers which are sensitive to the views and interests of the community and the staff at the Center which meets the needs for federally sponsored research.

The eventual number of federal employees that will be affected is not certain at this time. The total is affected by attrition, final organization structure, Congressional decisions, and discussions with outside entities. The present on-board strength (FTE) is 247 headquarters and 848 in the field. The FY 1983 request is for 100 at headquarters and 411 in the field. The affected employees are a total of 584 personnel. This number of contract employees in the field as a result of past defederalization and support is approximately 475 persons.

A preliminary estimate for all the Fossil Energy organization components including headquarters for RIF, severence pay, relocation, lump sum leave payments, etc. is approximately $20.0 million.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development
Engineering Analysis

QUESTION: The narrative states that no funds were requested for this line item in FY 1983 because responsibility is being transferred to the appropriate process-specific activity. Does this mean that the budget requests for those activities include amounts for engineering analysis, i.e., has the responsibility been transferred with commensurate funding? Are the engineering analysis funding levels embedded in the process-specific activities at the base level of $1.5M?

ANSWER: The FY 1983 budget request reflects the Department's position that large scale technology development should be the responsibility of the private sector. Prior year funding for engineering analysis primarily provided for engineering support that enhanced process scaleup and consequently is no longer necessary in view of the shift in the responsibility for large scale development activity. However, engineering analysis that is applicable to generic and long range R&D that has always been conducted within an activity is funded in the FY 1983 budget.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development Planning and Policy

QUESTION: This line item is a new $1.0M request for 1982. Why is there a need for increased (new) planning and policy formulation for a fossil energy program which is a quarter of its previous size?

ANSWER: Prior to the recent redirection of the Fossil Energy program, the program had a significantly different scope, which was reflected by the presence of a number of large-scale hardware tests. Efforts from the Planning/Policy group contributed significantly to the evolution of the previous program over the years. Prior to FY 1982, the yearly Planning/Policy budgets were significantly higher than that being requested in FY 1983. For example, the FY 1981 appropriation for planning and policy activities in Fossil Energy was $2.5 million. FY 1982 was a transitional year from old policy to new, and a decision was made to stretch out a number of prior year planning activities and not request any new funding for that year. Thus, the FY 1983 request should not be considered a "new" request, but rather a continuation of the previous program, although at a much-reduced level.

The new FE program has experienced a fundamental redirection to long-term, high-risk, potentially high payoff R&D. This redirection of program goals and objectives has created a variety of new planning/policy questions that must be dealt with relatively soon for the shift to occur in an efficient manner.

An additional consideration is that potential changes due to new perceptions of fossil fuel supply trends have made it necessary to re-examine the need and timing for new supplies of fossil fuels, and how Federal support of advanced technology R&D could affect this need. For these reasons the requested FY 1983 policy/planning funding is believed to be especially appropriate.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Research and Technology Development

QUESTION: Please provide a list of all the colleges and universities involved in the University Coal Research Program during FY 1982. Provide justification for the cancellation of the program.

ANSWER: The lists of awards in the University Coal Research Program for FY 1980 and FY 1981 are attached. Most of these projects were underway during FY 1982. The awards for FY 1982 have not yet been made; there were hundreds of proposals submitted in response to the NPI, and final review and selection of the approximately 25 winners is in progress.

The justification for cancellation of this program derives from the need to reduce federal expenditures and the existence of continuing university activities which are supported throughout the Fossil Energy R&D programs.

FOR IMMEDIATE RELEASE
AUGUST __, 1981

29 SCHOOLS TO BEGIN COAL RESEARCH UNDER A DOE FOSSIL ENERGY GRANT PROGRAM

Grants totaling $4.4 million were awarded today by the Department of Energy (DOE) to 29 universities across the country in an effort to increase their roles in the Federal coal research program. Of the 32 awards, 12 were follow-on renewals of awards made in the 1980 Program.

The universities are located in 21 states and Italy and will receive awards varying in value from $44,000 to $224,000 each.

DOE selected the awards from 305 proposals submitted five months ago following Congress' allocation of the research funds in the Department's Fiscal 1981 Appropriations Bill. Last December, more than 2500 university academic departments had received DOE's notice that it was interested in receiving proposals for university coal research projects.

Because of the large number of proposals involved, DOE limited its funding level for each to no more than $250,000, and to assure good regional distribution, no more than four grants were awarded to universities in a single state. There was an award to a foreign university, the University of Naples.

The projects will last from one to three years and will focus on specific research ares involving coal combustion, the conversion of coal into synthetic oil and gases, the characterization of coals from various regions of the country, and other related topics. The projects tend to stress fundamental aspects of coal research

(MORE)

For example, several schools will examine the way different solvents can affect the coal liquefaction process. Others will search for better catalysts for the reaction of synthesis gas to valuable fuels. Others will study various coal gasification and combustion phenomena in fluidized beds of coal. And in several projects, properties of coals from various regions will be studied using highly advanced analytical equipment and instrumentation.

In addition to developing new coal-related information that could ultimately help decrease the nation's dependence on foreign oil, the grants are also designed to encourage students and young faculty members to join senior professors in exploring innovative coal research projects.

The program will complement other DOE Fossil Energy research support to universities..

A list of the universities receiving grants, along with the principal investigators, proposal titles, funding amounts, and project durations, is attached.

-DOE-

News Media Contact: Robert C. Porter, 202/252-6503

EM-80-015

FE-WPI-81-002
PROPOSALS SELECTED FOR GRANT AWARD

University Principal Investigator	Grantee Share Amount	GOVT Share Amount	Months	Title
Purdue Research Foundation R. Viskanta, IN	0	$135,161.	24	"Thermal and Radiative Properties of Coal-Ash Deposits"
University of Utah A. Oblad, UT	$ 26,073.	$ 52,147.	12	"Homogeneously Catalyzed Hydrogenolysis of Coal Slurries, Coal-Derived Liquids and Model Compounds"
University of Kentucky J. Hower, KY	$ 10,000.	$146,856.	24	"Petrographic Characterization of Kentucky Coals"
Mississippi State University D. Murphree	$ 25,000.	$153,111.	12	"Research on Advanced Optical Techniques for Diagnostics, Control and Safety in Fossil Energy Processes"
University of Utah H. Meuzelaar, UT	0	$ 99,974.	12	"Characterization of Rocky Mountain Coals and Coal Liquids by Computerized Analytical Techniques"
Virginia Commonwealth University G. Nelson, VA	$ 38,985.	$130,351.	24	"New Catalysts for the Indirect Liquefction of Coal"
William Marsh Rice University J. Margrave, TX	0	$201,500.	24	"Studies of Initial Reaction Steps in the Gasification of Coal and Selected Model Compounds"
New Jersey Institute of Technology J. Liskowitz, NJ	$ 7,078.	$123,000.	12	"Development of Leachate Treatment Technology Utilizing Fly Ash as an Effective Low Cost Sorbent"
Western Kentucky University W. Lloyd, KY	$ 43,873.	$200,489.	24	"Predictors of Plasticity in Bituminous Coals"
University of Cincinnati J. Moteff, OH	0	$115,000.	24	"Correlation of Microstructure with the Pneumatic Erosion Behavior of Metals and Alloys"

PE-HPI-81-002
PROPOSALS SELECTED FOR GRANT AWARD

University Principal Investigator	Grantee Share Amount	GOVT Share Amount	Months	Title
North Carolina State University J. Ferrell, NC	$ 12,106.	$163,276.	24	"Pollutants from Coal Conversion Processes"
Pennsylvania State University R. Jenkins, PA	$ 6,832.	$129,795.	36	"Characterization of Porous Nature of Heat-Treated and Gasified Coals by Small Angle X-Ray Scattering"
University of California A. Bell, CA	0	$224,002.	36	"Conversion of Low H CO Ratio Synthesis Gas to Hydrocarbons"
Massachusetts Institute of Technology J. Beer, MA	0	$192,394.	24	"Freeboard Reactions in Fluidized Coal Combustion"
University of Naples L. Massimilla, Italy	$ 30,000.	$130,000.	36	"Carbon Attrition During the Fluidized Combustion and Gasification of Coal"
University of Rhode Island M. Knickle, RI	$ 17,100.	$190,600.	36	"Study of Multiphase Flow Useful to Understanding Scaleup of Coal Liquefaction Reactors"
University of Colorado W. Krantz, CO	0	$ 93,490.	24	"Modeling, Laboratory and Field Test Studies of Water Influx in the UCG Process"
Auburn University D. Williams, AL	$ 44,918.	$179,660.	24	"Solvent Tailoring for Coal Liquefaction Based on Critical Properties of Physical Solvating Agents and Hydrogen Donors"
Virginia Polytechnic Institute L. Taylor, VA	$ 48,417.	$106,357.	36	"Heteroatom Speciation in Coal Liquefaction via FT-IR Coupled With Liquid Chromatography"
Washington State University W. Grosshandler, WA	$ 12,099.	$125,000.	21	"Characteristics of Coal/Light Hydrocarbon Slurries in Spray Combustion (Phase II)"

PE-NPI-81-002
PROPOSALS SELECTED FOR GRANT AWARD

University Principal Investigator	Grantee Share Amount.	GOVT Share Amount	Months	Title
University of Pittsburgh G. Holder, PA	0	$190,000.	24	"A Basic Study of Supercritical Extraction of Model Compounds and Coal Liquefaction Residues, Supercritical Water to Convert Coal to Gases & Liq."
University of Texas at Austin M. Vasilakos, TX	0	$146,704.	24	"Solvent and Chemical-Reaction Effects in Supercritical Extraction of Coal"
Washington State University C. Crowe, WA	0	$ 44,089.	12	"Venturi Design for Metering Solids Flow in Gas-Solids Suspensions"
Lehigh University K. Klier, PA	$ 32,772.	$200,000.	36	"Catalytic Combustion of Coal and Synthetic Fuels"
Carnegie-Mellon University E. Suuberg, PA	$ 5,302.	$104,538.	24	"Characterization of Tar Products from Pyrolysis of Bituminous Coals and Development of a Physically Realistic Model of Softening Coal Pyrolysis"
University of Illinois at Urbana-Champaign M. Chen, IL		$249,979.	30	"Measurement of Solids Motion in Gas Fluidized Beds"
Western Michigan University T. Houser, MI	$ 23,137.	$105,462.	24	"Fundamentals of Nitric Oxide Formation in Fossil Fuel Combustion"
Cornell University P. de Boer, NY	0	$ 98,335.	24	"Pyrolysis of Coal-Derived Fuels Using the Laser-Powered Homogeneous Pyrolysis Technique"
Washington University B. Joseph, MO	0	$101,391.	24	"Theoretical and Experimental Studies of Fixed-Bed Coal Gasification Reactors"

FE-NPI-8:-002
PROPOSALS SELECTED FOR GRANT AWARD

University Principal Investigator	Grantee Share Amount	GOVT Share Amount	Months	Title
Washington State University R. Filby, WA	$ 10,000.	$124,410.	24	"Chemistry of Mercury in Coal Hydrogenation and Waste Treatment"
Southern Illinois University P. Davis, IL	$ 6,631.	$137,647.	24	"Flow and Design Characteristics of the Hydrocyclone for the Recovery of Coal Fines"
University of Alaska, Fairbanks P. Rao, AL	$ 5,645.	$ 50,803.	18	"Petrology of Cretaceous Coals from Northwestern Alaska — Phase II"

U.S. DEPARTMENT OF ENERGY
OFFICE OF PUBLIC AFFAIRS
WASHINGTON, DC [illegible]

DOENEWS:

FOSSIL

FOR IMMEDIATE RELEASE
JUNE 19, 1980

41 SCHOOLS TO BEGIN COAL RESEARCH UNDER $5 MILLION DOE GRANT PROGRAM

Grants totaling $5 million were awarded today by the Department of Energy (DOE) to 41 universities across the country in an effort to increase their role in the Federal coal research program.

The universities are located in 26 states and will receive awards varying in value from $40,000 to $240,000 each.

DOE selected the universities from 530 that submitted proposals four months ago following Congress' allocation of the research funds in the Department's Fiscal 1980 Appropriations Bill. Last December, more than 2500 university academic departments had received DOE's notice that it was interested in receiving proposals for coal research projects.

Because of the large number of proposals involved, DOE limited its funding level for each to no more than $250,000, and to assure good regional distribution, no more than three grants were awarded to universities in a single state. At least one grant was awarded to a university in each major coal region.

The projects will last from one to three years and will focus on specific research areas involving coal combustion, the conversion of coal into synthetic oil and gases, the characterization of coals from various regions of the country, and other related topics.

(MORE)

For example, several schools will examine the way different catalysts can speed chemical reactions involving coal. Others will study various combustion phenomena, such as the way coal behaves when burned in the turbulent motion of a "fluidized bed combustor." And in several projects, properties of coals from various regions will be studied using highly advanced analytical equipment and instrumentation.

In addition to developing new coal-related information that could ultimately help decrease the nation's dependence on foreign oil, the grants are also designed to encourage students and young faculty members to join senior professors in exploring innovative coal research projects.

The program will complement other DOE Fossil Energy research support to universities.

A list of the universities receiving grants, along with their proposal title, funding amounts and project duration, is attached.

-DOE-

News Media Contact: Robert C. Porter, 202/252-6503

SM-80-015

1980

NPI PROPOSALS SELECTED FROM HIGH RANKED/RATED RESEARCH AREA PROGRAM RECOMMENDATIONS

NPI Coal Research Area	Proposal #	Principal Investigators	University Department	Proposal Title	Funding Amount,$	Funding Years
A. Combustion of Coal and Synfuels	040	K. W. Ragland	U. of Wisconsin Mech. Eng. Madison, WI 53706	Coal Combustion in High Convective Flows	80,190	2
	130	J. C. Chen	Lehigh Univ. Mech. Eng. Bethlehem, PA. 18015	Heat Transfer to Tubes in Freeboard Space of Fluidized Bed Combustors	199,830	2.25
	367	J. M. Beér	M.I.T. (Mass.) Chem. Eng. Cambridge MA. 02139	The Investigation of the Freeboard Reactions in Fluidized Coal Combustion	57,808	1
	520	W. L. Grosshandler C. Crowe J. Chung	Wash. State Mech. Eng. Pullman, WA 99164	Characteristics of Coal/Light Hydrocarbon Slurries in Spray Combustion	125,000	1
	406	P.C.T. deBoer	Cornell Mech. & Aero Eng., Ithaca, NY. 14853	Pyrolysis of Coal-Derived Fuels Using the Laser-Powered Homogenous Pyrolysis Technique	49,843	1
	305	L. Kennedy E. Ruckenstein	SUNY-Buffalo Mech/Chem. Eng. NY. 12201	Catalytic Combustion of Synthetic Fuels	95,999	1
B. Characterization Related to Processes	190	J. C. Hower	U. of Kentucky Inst. of Mining/ Mineral Res. Lexington, 40506	Petrographic Characterization of Kentucky Coals	68,269	1

NPI PROPOSALS SELECTED FROM HIGH RANKED/RATED RESEARCH AREA PROGRAM RECOMMENDATIONS

NPI Coal Research Area	Proposal #	Principal Investigators	University Department	Proposal Title	Funding Amount,$	Funding Years
	245	R. J. Pugmire D. M. Grant	U. of Utah Fuels Eng/Chem Salt Lake City UT. 84112	Characterization of Western Coals by Solid State NMR	199,873	3
	438	K. E. Starling	U. of Oklahoma Chem. Eng. & Mat. Science Norman, OK 73019	Self-Consistent Thermodynamics & Transport Property Correlation Framework	[illegible]	2
	305	A. R. Tarrer J. A. Guin	Auburn Univ. Chem. Eng. Auburn, AL 36830	Solvent, Tailoring in Coal Liquefaction Based on Critical Properties of Physical Solvating Agents and Hydrogen Donors	85,000	1
	210	B. R. Cooper	West Va. Univ. Physics Morgantown, WV 26506	Surface Electronic Properties Study of Catalytic Effectiveness and Poisoning in Fuel Synthesis	138,277	2
	173	J. S. Shabtai A. G. Oblad	U. of Utah M. & F.E. Salt Lake City UT 84112	Catalysts for Upgrading Coal Liquids - Synthesis & Functionalization of Large-Pore Molecular Sieves	100,000	1
	247	H. L. Meuzelaar J. Futrell G. Hill	U. of Utah Chemistry Salt Lake City UT 84112	Characterization of Rocky Mountain Coals and Liquids by Computerized Analytical Techniques	99,998	2
	065	P. D. Rao	U. of Alaska Mineral Ind. Res. Lab(Chem) Fairbanks AK 99701	Petrology of Cretaceous Coals from Northwestern Alaska	49,981	1

NPI PROPOSALS SELECTED FROM HIGH RANKED/RATED RESEARCH-AREA PROGRAM RECOMMENDATIONS

NPI Coal Research Area	Proposal #	Principal Investigators	University Department	Proposal Title	Funding Amount,$	Funding Years
C. Structure, Reactions and Analysis	417	P. R. Griffiths	Ohio State Chemistry Athens, OH 45701	Investigation of the Changes of Coal Structure Occurring During Gasification	99,958	2
	236	N. Peppas	Purdue Univ. Chem. Eng. W. Lafayette IN. 47907	Macromolecular Structural Changes in Bituminous Coals	107,043	3
	002	J. W. Larsen J. Kovac	U. of Tenn. Chemistry Knoxville TN. 37916	Coal Structure & Reactivity	168,002	2
	229	A. S. Pavlovic J. J. Renton	W. Va. U. Physics Morgantown W.V. 26566	X-Ray Diffraction & Scattering Studies of Coal Constituents	144,734	3
	096	N. Woolsey D. Bartak	U.of N.Dakota Chemistry Grand Forks ND. 58202	Quantitation of the Reductive Cleavage of Aryl Ethers of SRC & Coals	99,993	2
	374	G. Melson	VA Common-wealth; Chem. Richmond VA. 23284	New Catalyst for the Indirect Liquefaction of Coal	119,845	1
	369	W. A. Peters J. P. Longwell	M.I.T. Chem. Eng. Cambridge MA. 02139	Thermal Reactions of Aromatics in the Presence of Calcium Oxide	200,000	2

NPI PROPOSALS SELECTED FROM HIGH RANKED/RATED RESEARCH AREA PROGRAM RECOMMENDATIONS

NPI Coal Research Area	Proposal #	Principal Investigators	University Department	Proposal Title	Funding Amount,$	Funding Years
	313	V. F. Yesavage A. J. Kidnay	Colorado Sch. of Mines Chem. Eng. Golden, CO. 80401	Phase Equilibrium Properties of Coal Derived Liquids	177,972	2
	007	H.H. Kung	Northwestern Chem. Eng. Evanston, IL. 60201	Mechanistic Studies of Catalytic High Pressure Methanol Synthesis	136,836	3
D. Multiflow Phenomena	319	C. Crowe	Wash. State Mech. Eng. Pullman, WA. 99164	Venturi,Design for Metering Solids in Gas-Solid Suspension	44,073	1
	295	G. M. Homsy	Stanford Chem Eng. Stanford, CA. 94305	Particle Circulation & Solids Transport in Large Bubbling Beds	153,134	2
	291	D. E. Clough	U. of Colo. Chem. Eng. Boulder, CO. 80309	Time-Dependent Behavior of Large Particle Fluidized Bed Hydrodynamics	69,000	2
E. Reactor Engineering	410	T.J. Fitzgerald	Oregon State Chem. Eng. Corvallis, OR. 97331	Testing of Cold Sealed Bed Modeling for Fluidized Bed Combustors	99,842	1
	132	B. Joseph	Wash.U.,Mo. Chem. Eng. St. Louis, MO. 63130	Dynamic Behavior of Moving Bed Coal Gasification Reactors	39,700	1

NPI PROPOSALS SELECTED FROM HIGH RANKED/RATED RESEARCH AREA PROGRAM RECOMMENDATIONS

NPI Coal Research Area	Proposal #	Principal Investigators	University Department	Proposal Title	Funding Amount,$	Funding Years
	324	J. M. Calo	Princeton U. Chem. Eng. Princeton, NJ 08544	Heterogeneous Kinetics of Coal Gasification	182,036	3
	039	Y. T. Shah	U. of Pgh. Pet./Chem.Eng. Pittsburgh, PA 15261	Experimental Study of the Multiple Steady States in Adiabatic Coal Liquefaction Reactor	150,000	2
F. Environmental Aspects	281	R. G. Luthy	Carnegie Mellon Chem. Eng. Pittsburgh, PA 15213	Physicochemical Adsorption Phenomena of PAH in Coal Conversion Wastewaters	101,588	2
	407	J. M. Henry	Tulane Univ. Chem. Eng. New Orleans LA 70118	Simultaneous SO_2 & NO_x Flue Gas Cleaning	130,101	1.5
	083	J. Liskowitz	NJ Ins./Tech. Chemistry Newark, NJ 07102	Parameters that Control Sorbent Characteristics of Fly Ash	127,277	1
	215	J. K. Ferrell	N.C. State Chem. Eng. Raleigh, NC. 27650	Pollutants from Coal Conversion Processes	86,724	1
	118	M. Suidan	U. of Illinois Civil Eng. Urbana, IL. 61801	Treatment of Coal Conversion Wastewater with PAC Contact Stabilization Activated Sludge Process	130,000	2

NPI PROPOSALS SELECTED FROM HIGH RANKED/RATED RESEARCH AREA PROGRAM RECOMMENDATIONS

NPI Coal Research Area	Proposal #	Principal Investigators	University Department	Proposal Title	Funding Amount,$	Funding Years
G. Miscellaneous	016	R. Kletzien	W. Va. Univ. Chemistry Morgantown WV 26506	Assessment of Carcinogenic Potential of Products Produced by Coal Conversion Processes	49,002	2
	490	E. R. Hoskins J. E. Russell	Texas A&M Petro. Eng. College Station TX 77843	Geotechnical Studies Related to In Situ Lignite Gasification Trials	219,526	3
	120	R. H. Yoon F. Sebba	VPI & SU Min./Mineral Chem. Eng. Blacksburg VA 24061	Application of Microgas Dispersion for Fine Coal Cleaning	149,788	2
	338	Monte Hall	So. Ill. U. Chemistry Carbondale, IL 62901	Selective Grinding of Coal for Efficient Pyrite Removal	81,774	1.25
	193	H. Conrad	U. of Ky. Metallur. Eng. Lexington, KY 40506	Mechanisms of Fatigue in Metals Under Coal Liquefaction Conditions	199,791	3
	130	R. D. Clark	New Mexico Highlands U. Science & Math Las Vegas, NM 87701	Modification of Coal Fly Ash Size Distribution	199,970	2

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Control Technology and Coal Preparation
Coal Preparation and Analysis

QUESTION: With respect to the microwave and fused salt cleaning processes, what would be the next step if these current activities prove successful? What role will the Federal Government play beyond current activities?

ANSWER: The next step would be an engineering evaluation of principal design and operating variables leading to design selection, assembly, and experimental operation of "proof-of-concept" prototypic equipment, or "PDU". A Federal role is necessary to carry the work through completion of this "proof-of-concept" phase. This will provide the essential base technology required by the private sector to assess economic and performance factors, upon which commercial scale development determinations would be made. No Federal role is envisioned in this "commercial development" phase. Private sector cost sharing during later stages (84-87) of the "proof-of-concept" phase may be possible, depending upon development progress in coal-water slurry and other advanced coal utilization technologies and favorable market study projections.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Control Technology and Coal Preparation
Flue Gas Cleanup

QUESTION: Does this program include activities directed toward investigations which would provide an understanding of the relationship between the combustion process and acid precipitation? The program narrative states that "primary needs are to develop cleanup systems which are economic and which will operate reliably and efficiently over a broad range of coals and load conditions." Does this statement mean to imply that current technology (precipitators, bag houses) are not economic or reliable?

ANSWER: This program is primarily concerned with acquiring basic data for the control of potentially harmful combustion emissions such as NO_x, SO_x and particulates which are believed to be important precursors to the formation of acid precipitation. Activities addressing the transformation of emissions to acid precipitation are conducted by DOE groups outside of Fossil Energy. These activities are being closely monitored by our Program Managers so that if we identify the need to more closely control some of these emissions we will be able to do so effectively and economically.

The statement concerning the need for more economic and reliable cleanup systems pertains to our efforts to develop simultaneous NO_x/SO_x reactors rather than improved particulate control systems and does not mean to imply that currently available cleanup systems are not economic or reliable. The particulate control portion of our present program constitutes only a small portion of the overall Flue Gas Cleanup Program and consists primarily of a modest effort in the application of electron irradiation for particulate removal at Florida State University. This work could lead to the integration of particulate cleanup into the simultaneous NO_x/SO_x reactors as discussed above with significant economic benefit and perhaps with enhanced removal of the small respirable particles.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Control Technology and Coal Preparation
Gas Stream Cleanup - Turbine Cleanup Systems

QUESTION: The narrative states the program performance objective is to develop hot gas cleanup systems by 1985 which are capable of reducing particulate levels to 0.012g/SCF and alkali levels of 0.02 ppm. It also notes that "there are no commercially available processes for alkali vapor removal." Yet the 1983 request is $1.5M compared to a base of $8.3M. With this funding level, what are the prospects for attaining the stated program goal?

ANSWER: The effects of the budget cuts on the stated program goals are primarily two-fold:

1. The scale at which gas cleanup system performance is measured will be reduced. Proof-of-concept testing will be limited to the Government-owned pressurized fluidized bed combustion small gas turbine (PFBC-SGT) located in Woodridge, New Jersey. Plans for scale-up and evaluation at the pilot plant level have been cancelled. Concept development currently ongoing at the lab/bench scale level will be completed and results documented.

2. With respect to attaining the particulate performance goal, the budget reduction would have a minimal effect other than reducing the scale at which performance will be demonstrated as discussed previously. Our capability to achieve the alkali cleanup goal will, however, be impacted in as much as we will have to reduce the number of approaches under investigation.

In summary then, we anticipate that the reduced funding level could cause delay in successfully meeting our alkali cleanup goals and that for both alkalis and particulates, determination of cleanup performance will be made at a scale smaller than originally contemplated. However, it does not necessarily follow that reduced funding will affect the ultimate commercial development of these technologies. Activities of the private sector and energy prices are also important factors influencing the timetable for technology development. In the final analysis, the market must eventually set the timetable for private sector scaleup and commercial development in this area.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Control Technology and Coal Preparation Process Modification Technology

QUESTION: Does this represent a new program or a transfer? What specific activities have been identified for potential funding?

ANSWER: This is not a new program or transfer. This program was initiated in FY 1979 and funded in FY 1980 and FY 1981. As a result of national and international concern regarding the acid rain problem, the Administration reoriented this program during the FY 1982 budget projection process to allow for participation with the EPA on the limestone injection/multistage burner (LIMB) concept. DOE funding for this concept was not approved by Congress for FY 1982. However, several of the other projects initiated in the FY 1979-FY 1981 time frame were continued in FY 1982 using prior year funding.

For FY 1983, we would plan to fund exploratory and applied research in two areas:

1. Determination of the kinetics and mechanism of formation of various forms of sulfur, nitrogen, and hydrocarbons produced in staged coal pyrolysis/combustion, in the absence and presence of selected sorbents.

2. Determination of the kinetics and mechanism of formation of H_2S, NH_3, HCl, and Na_2SO_4 in the staged pyrolysis/partial oxidation of coal to low-Btu gas, in the absence and presence of selected sorbents.

This work will hopefully provide a better scientific base for the subsequent development of coal gasification and combustion process modifications involving in-process pollutant control which has the potential of being a more cost effective approach than post-process cleanup trains.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Combustion Systems
PFBC

QUESTION: Last year, the Committee was advised that the Grimethorpe facility operating cost for DOE would be $8M. The FY 1983 request is $2.5M for data and a report. Does the level of this request imply a withdrawal from facility operations? What is the dollar amount of the remaining U.S. contractual commitment to IEA/Grimethorpe?

ANSWER: The U.S. remains committed to the completion of a viable program at Grimethorpe. We believe that a test program can be devised that will still provide adequate and substantial data to the United States, United Kingdom, and the Federal Republic of Germany. The $2.5 million requested in FY 1983 is required to complete a planned test program and to prepare final technical reports of the test program. This amount represents the final U.S. contractual commitment to IEA/Grimethorpe.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Combustion Systems

QUESTION: Given the state-to-state nature of IEA projects, is there much possibility that U.S. companies or EPRI will participate in this project in the future? What are the plans of the U.K. and F.R.G. for the project when the U.S. withdraws? Does termination of U.S. involvement indicate that DOE does not think further research at Grimethorpe with an integrated turbine is necessary?

ANSWER: DOE has received a proposal from the IEA project for a Gas Turbine Test Program at the Grimethorpe facility. This test program would begin upon completion of the present implementing agreement. The proposal is for a five-year period at an estimated cost of $29.75M pounds (1982 dollars at current exchange rate is $55.0M).

DOE is currently circulating this proposal to some 48 companies and organizations (such as EPRI) known to be interested in the Pressurized Fluidized-Bed technology to assess the degree of interest they have in participating in this phase of the Grimethorpe project.

The U.K. and FRG are asking their industrial sectors if they are interested in participating financially in the project. In addition, the Operating Agent, National Coal Board of the U.K., with the help of IEA is investigating the interest of additional countries as sponsors to further reduce the individual cost of participation in the proposed Gas Turbine Test Program at Grimethorpe.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Combustion Systems
Alternative Fuel Utilization

QUESTION: The narrative states that "successful development of these (mixture) fuels could eventually result in the replacement of over 3MMBD of oil." What time frame is envisioned for the realization of this goal? Does the budget request represent a sufficient level of effort to realize such a goal?

ANSWER: Liquid-coal mixture technology has potential application in three principal areas representing increasing technical severity and risk; coal-designed equipment which is currently oil-fired, industrial applications where installation of traditional coal-firing equipment is precluded by constraints associated with solids handling and storage requirements, and retrofit of emplaced oil-designed equipment. The degree to which this technology becomes both technically and economically viable in these applications depends on the degree to which the technical issues, which are the subject of the current R&D programs are successfully resolved. It is anticipated that coal water mixtures represent a mid-term potential (1985-1990) for achieving the stated goals in regard to replacement of oil. The budget request represents the minimum resource requirement to obtain the necessary data and to perform the required supporting technology development over a four year period required in support of these stated goals. For a significant portion of these applications, fuel production and distribution system requirements for this technology are anticipated to involve adaptation of existing equipment at modest capital cost. For this reason, assuming resolution of the technical utilization issues, implementation time is expected to be relatively short.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Fuel Cells

QUESTION:	Regarding the proposed funding levels for activities in which GRI acts as a co-sponsor, how quickly will GRI be able to increase its funding of such activities?
ANSWER:	The Gas Research Institute (GRI) is planning on committing approximately $8 million in 1982 for the 40KW field test and $2 million for technology development at United Technologies for KW-size On-Site/Integrated Energy Systems. In 1981, the Department urged GRI to increase their financial support of fuel cell activities, and as a result, GRI did increase their support in 1981 and 1982. Approximately 12 percent of GRI's budget is presently allocated to phosphoric acid fuel cells. It is unknown if GRI will be able to further increase their financial support in 1983.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Fuel Cells

QUESTION: Of the funds for phosphoric acid technology, what will be the division of funds among United Technologies Corporation (UTC), Westinghouse, Engelhard and NASA-Lewis overhead? Please provide this breakout for FY 1982 as well. What is each of these participants expected to deliver for these funds?

ANSWER: For FY 1982, DOE/NASA plans the following funding for phosphoric acid fuel cell technology development.

	NASA Carryover On 1/1/82 ($000)	DOE Allocation FY 1982 ($000)	Total To Be Obligated FY 1982 ($000)
UTC Electric Utility	3,126	6,000	9,126
Westinghouse Electric Utility	0	4,700	4,700
UTC 40 KW R&D	3,000	1,300	4,300
Engelhard OS/IES R&D	0	2,900	2,900
NASA Personnel, Travel, DCAA[1/]	97	950	1,047

[1/] NASA is phasing out support for phosphoric acid fuel cells. No NASA support is presently planned for FY 1983.

For FY 1983, $4 million is planned for technology development at Westinghouse and United Technologies for electric utility systems. No funding is planned for On-Site technology development (Engelhard and UTC). The division of funds between Westinghouse and United Technologies has not yet been determined. However, a description of the planned activities for each firm follows.

United Technologies will continue development efforts on the higher pressure (120 psia), large area cell and stack. Twenty cells, which are ten ft. sq. in size and are configured in 120 psia short stacks will be tested to verify technology and manufacturing cost reduction improvements. No full scale (454 cell) stacks will be fabricated. At Westinghouse, efforts will continue, at a reduced pace, on the development, fabrication and testing of 10KW short stacks.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Fuel Cells

QUESTION: What is the anticipated duration of the 4.8MW powerplant test program? What is the cost-share Federal obligation?

ANSWER: The present schedule plans for operational testing of the 4.8KW powerplant to occur from July, 1982 to approximately October, 1984. Presently, the powerplant is undergoing pre-operational checkout. DOE's FY 1983 funding will conclude DOE's contractual obligation for this project with the exception of the restacking of the power section. DOE's FY 1982-FY 1983 funds identified for restacking the 4.8MW power plant have been used instead to fund cost overruns. The balance of the identified funds have been used for technology development necessary for possible future restacking. DOE does not, at this time, have a firm cost proposal for restacking.

The DOE funding presently planned and programmed for completion of the test program through FY 1983 is as follows:

	DOE Share	EPRI Share	Con-Ed Share	UTC Share
UTC Manufacture, Fabrication	$35.4M	$ 7.2M	$ ...	$13.4M
UTC Spare Parts Contract	2.9M	...	...	...
Con Edison - Siting & Testing	7.5M	7.5M	5.8M	...
TOTAL	$45.8M	$14.7M	$ 5.8M	$13.4M

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Fuel Cells

QUESTION: Does the Agency believe that the request for the 4.8MW powerplant will lead to "commercial demonstration"?

ANSWER: The 4.8MW powerplant is a pre-prototype phosphoric acid electric utility fuel cell powerplant. As such, it is not a commercial prototype, however, the experience gained and the technology developed and tested will greatly aid the ultimate commercialization of this technology. Presently the manufacturers and interested electric utilities are conducting pre-negotiations for the delivery of nominal 10MW prototype powerplants in the mid to late 1980 time period. The experience derived from the fabrication, siting, checkout and anticipated 1982 operation of this powerplant has contributed significantly to these pre-negotiation discussions. In addition, the powerplant will, during its operating life, serve as a test bed where advanced components of a prototype powerplant can be tested. An example of this is the power conditioning system.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

MHD

QUESTION: The narrative states that "MHD is a long term technology which, relative to other Agency funded electricity generating technologies under development, has much higher development risks and costs." How does this statement fit with the stated Agency philosophy of supporting high-risk, long-term R&D?

ANSWER: The Agency's philosophy is indeed to support high-risk, long-term R&D activities. However, in light of the Administrasion's commitment to fiscal responsibility and the necessary fiscal austerity, it is not possible to support all of the fossil energy technologies that have been pursued in the past. Within the latter framework, every effort is being made to develop and implement the most effective fossil energy program possible. MHD was one of the programs proposed for termination in order to achieve the Administration's fiscal goals. The rationale for terminating MHD in FY 1982 is that MHD is a technology that requires a large-scale, proof-of-concept demonstraion with attendant high development cost to prove that the technology can progress to a commercial stage. In addition, other energy conversion technologies being pursued in Fossil Energy such as pressurized fluidized bed combustion, fuel cells, and combined cycle gasification hold much of the same promise as MHD at a lower overall development cost and risk. In light of the budgetary situation, the need for fiscal responsibility, and the multiple energy conversion technologies presently being pursued within the Agency's Office of Fossil Energy and the industrial sector, the Administration has decided not to support continuation of funding for MHD.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

MHD

QUESTION: Please provide the plans for close-down of the major facilities, CDIF, CFFF and the Arnold Engineering Development Center (AEDC)? When will they be closed? What are mothballing and termination cost estimates for each? How many personnel will be affected and what will be the separation costs? What component development activities will be prematurely terminated?

ANSWER: Concurrent with the termination of the MHD program in mid-April 1982, stop work orders will be issued to the major facilities. The facilities will be placed in a mothballed condition and the staffs will be reduced to those necessary to maintain security and protect government equipment from undue deterioration. A determination will be made for each facility whether it can continue to be utilized by the Government or will be otherwise dispositioned. Mothballing and termination costs are projected to be $2.4 million at CFFF, $4.4 million at CDIF, and $0.3 million at the Arnold Engineering Development Center (AEDC). Approximately 135 people at CFFF, 120 people at CDIF, and 25 people at AEDC will be directly affected by the stop work orders. Separation costs have been considered in the above termination estimates. All development activities will be brought to an orderly conclusion.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Advanced Process Technology - Advanced Exploratory Research

QUESTION: To what degree are research efforts in this program directed toward health and environmental aspects complement those in other programs, i.e., coal liquefaction? The narrative describes activities "directed toward development of new/improved methodology and instrumentation for measuring critical parameters for the extraction of gas from tight reservoirs." Is this consistent with the reasoning behind terminating the unconventional gas recovery program which holds that private industry has both the financial and technical resources to develop this technology?

ANSWER: The principal candidates for sizeable synfuels production by the year 2000 are shale oil and coal liquids. Shale oil may precede coal liquids because of economics. Fossil Energy's Advanced Process Technology program on health and environment centers largely on shale oil which complements work on EOR and coal liquids. Generic research on shale oil seeks to identify the basic chemical characterization of those components presenting health hazards such as nitrogen, sulfur, arsenic and vanadium. These areas of interest are also relevant to very heavy oil and coal liquids chemistry. All three areas present problems of possible air and water pollution without near term ultimate solutions.

With regard to the rationale behind termination of the unconventional gas program, Fossil Energy areas of research represent generic activities not expected from industry, such as:

1. Modeling of massive hydraulic fracturing.
2. Sonic and electrical properties of tight gas sands.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Enhanced Oil Recovery

QUESTION: What is the prognosis for the future based upon the long term downhole steam generator test? What is the current state-of-the-art of tar sand technology? What U.S. firms are involved in developing the technology? The rationale for zeroing-out the tar sand activity refers to oil decontrol, tax incentives, the SFC incentives, etc." Is this same rationale not applicable to the heavy and light oil activities, i.e., since the incentives are in place, why was the tar sand activity singled out for termination?

ANSWER: The future looks very good for the downhole generator and we hope that by the time our field testing is completed at the end of this fiscal year, there will be several private companies willing to continue development and market the product. The concept of generating and injecting steam and exhaust gases downhole has been proven sound by our tests. These tests show that one can achieve exceptionally high thermal efficiency while at the same time eliminating surface air pollution with the downhole steam generator. The high thermal efficiency results from elimination of heat losses encountered in surface steam generation. Heat lost by steam while travelling down the well and that lost up the stack of a boiler are the principal sources of this heat loss. Surface air pollution is eliminated because both NO_x and SO_x compounds are either converted into water soluble salts or are otherwise scrubbed within the reservoirs.

Many major oil companies have some type of project involving tar sand development. Since about 90 percent of the domestic tar sand resource cannot be surface mined (as in Canada), we have to develop recovery techniques similar to some of the enhanced oil recovery processes that can be applied in place. Technology for recovery of tar sand oil is in its infancy with the exception of some pilot-stage work performed recently. However, with the number of companies interested in development, we expect future growth in this area will continue, as Federally owned resources are made available to industry and the process economics improve.

Although such an impression is created, the tar sand activity was not singled out for termination. The petroleum R&D program was realigned away from field tests and demonstration projects and directed more towards long-range, high-risk efforts. Because of the relatively smaller size of the tar sand resource and the nature of existing projects, it did not fit in this new strategy. However, since many of the new initiatives in unconventional oil are basic-research oriented, the results from these projects will be applicable to tar sand resources also. In addition, our cooperative agreements with Venezuela and especially Canada, will provide for a continuing DOE presence in this area.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Oil Shale

QUESTION: The budget book narrative states that "It is generally anticipated that the first plants can comply with current environmental standards with available technology." A GAO report on oil shale development, updated in December, 1981, states "while there is reason to think that some of these (environmental) control technologies will work, they have not yet been tested commercially." Does the Agency have any plans to collect environmental and health-related data at the Colony and Union projects to better understand environmental phenomena at the commercial size?

ANSWER: Both Union and Colony are using a zero water discharge approach, thereby eliminating it as a factor. However, uncontrolled water discharge must be considered and samples will be taken of the process water. Also, samples of water used for spent shale cooling and compaction will be taken to relate to vapors monitored. They have obtained Mine Land Reclamation Board permits requiring the companies to do research to better define key parameters. The financial assistance agreements incorporate R&D plans on solid waste management as requirements. The Oil Shale Program Office in conjunction with the Office of Energy Research's Office of Health and Environmental Research program offices will be reviewing the R&D plans for the state. As to air emissions, only regulated pollutants are addressed in the Prevention of Significant Deterioration (PSD) permits (SO_x, NO_x, CO, HC, and particulates). It is widely believed that available technology can handle these emissions. Other elements, however, will be monitored by Union and Colony as required under the agreement. DOE is planning to ask the SFC to assign its rights to obtain samples to DOE. We are therefore planning to collect and analyze much environmental and health-related data.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Oil Shale

QUESTION: The environmental mitigation research activity seems to be primarily concerned with retorted shale disposal and air emission control. A 1981 study by the MITRE Corporation on synthetic fuel health and environmental effects describe many unknowns requiring further research.

Is the Agency's proposed program adequately financed to undertake the diversity of research necessary to produce a near-term understanding of these effects enabling implementation of corrective and control measures prior to large-scale development?

ANSWER: The MITRE report addressed, in addition to the spent shale and air emission areas, health effects (definition of exposure, risk assessment, industrial hygiene, epidemiology); aquatic effects (process water discharges, product and process spills, leaching from spent shale, airborne deposition, hydrologic modification, ecological test and evaluation); and terrestrial effects (liquid product/waste water spills, land use). The DOE research program does in fact include a significant effort in almost all these areas. But the major effort is in the Office of Energy Research's Office of Health and Environmental Research (OHER) program where they are spending upwards of $5 million. The Oil Shale Program activity is also doing research in these areas. For example, the Lawrence Berkley Labs are conducting about $300,000 worth of water control technology research, exploring conventional and bioreactivity approaches. The Laramie Energy Technology Center also has about a $300,000 level of effort in water clean-up research. The Pacific Northwest Labs are evaluating and quantifying the organics of retort water. OHER is sponsoring health effects research covering the entire spectrum noted above.

As indicated previously, many of the questions as to environmental impact and health effects cannot be answered until commercial scale facilities are brought on-line. Our program is adequately funded to do the exploratory research to define options and determine the data that must be obtained. The financial assistance agreements are structured to provide much of the needed information.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Oil Shale

QUESTION:	A June, 1980 study by the Office of Technology Assessment on Oil Shale Technologies noted that the current dispersion models utilized in emissions prediction studies have been developed primarily for flat terrain. The oil shale deposits, however, are located in the mountainous west, and OTA identified a need for further research in this area. What is the state-of-the-art of our ability to predict the effects of synfuel plant sitings and with respect to the OTA study, does the stated deficiency exist and, if so, what is being done to rectify it?
ANSWER:	A good dispersion model does not exist; therefore, the effects of plant sitings cannot be adequately determined. The Office of Energy Research's Office of Health and Environmental Research is currently moving its Atmospheric Studies in Complex Terrain program (air model effort) from the Geisers area of North California to Oil Shale country (N.W. Colorado). This program will try to produce a rough terrain dispersion model that will satisfy the needed requirements.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

Oil Shale

QUESTION: What research effort is directed toward trace metals and toxic organic substances? What is the requested funding?

ANSWER: The Agency has a balanced program dealing with trace metals and organic substances. Two organizational elements sponsor complementary work in this area. Fossil Energy concerns itself with research in mitigating adverse environmental affects in specific Fossil Energy applicaions whereas the Office of Energy Research's Office of Health and Environmental Research (OHER) concerns itself with such broad environmental issues as ecological impacts, health and safety guidelines, data base development for air and water, and definition of solid waste problems. OHER activities principally involve basic scientific research and have broad scientific applicability.

Fossil Energy is planning the following trace metals and organics program in FY 1983:

Activity	Location	Funding
Organic toxicity and trace metals in air, water, retorted shale	LETC	$340,000
Trace metals in leachate from retorted shales	LANL	200,000
Organic vapors emitted from cooling spent shale with retort water	LBL	300,000

The following research in trace metals and organics is planned by OER in FY 1983:

Activity	Location	Funding
Oil Shale Effluent Characterization	PNL	$360,000
Analytical Chemistry	LANL	50,000

Activity	Location	Funding
Environmental Fate of Biologically Active Trace Contaminants	LLL	160,000
Sulfur Containing Heterocycles	Brigham Young	128,000
Processes Influencing Form, Chemical and Microbiological Stability, Mobility of Base Metals and Spent Shale	PNL	$325,000
Trace Chemical Concentrations in Stream Sediment, Soil, Water and Plants	U. of Colorado	210,000
Organic and Trace Toxicity of "Product Waters", Oil Shale Pollutants and Inhalation Studies	LANL	860,000
Shale Dust-Diesel Particulates	LANL	200,000
Trace Metals in Leachate from Retorted Shales	LANL	200,000
Organic Vapors Emitted from Cooling Spent Shale with Retort Water	LBL	300,000
TOTAL		$3,133,000

HS&T Hearing
Energy Development
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Asked by Don Fuqua
For Hearing 3/18/82

General

QUESTION: The Committee is aware of recent, abrupt changes in funding philosophies in fossil energy. Whereas earlier activity had been directed largely to the design, construction, and operation of pilot and demonstration facilities, the present funding requests are pointed to "performing more generic and technology-based research." Due to the fact that DOE presently has basic research management capability in the Office of Energy Research, what is the justification for continuing the fossil energy activity as a separate entity and in this new role? It would appear that more efficient use of manpower and other resources would be accomplished by moving fossil energy research programs into the Office of Energy Research. Please comment.

ANSWER: While Fossil Energy and the Office of Energy Research are both concerned with fostering the development of generic and technology base research, the emphasis of both programs is quite different. It is this difference which justifies maintaining them as separate but complementary activities.

"Technology base" can be thought of as a continuous spectrum of research, consisting of basic research, applied research, and exploratory development. The role of Energy Research is at the more basic end, while Fossil Energy in general concentrates on more applied research and exploratory development. The two approaches differ not only in research content, but also in the time scale for potential application. The Energy Research program is the longer range, concentrates predominantly on knowledge and scientific insight, and is done almost exclusively at universities and national laboratories.

The Fossil Energy program by contrast sponsors generic and technology base R&D or research that is specifically directed at fossil fuels, associated processes, and utilization. The orientation of the program is directed toward applied research. Consistent with the fact that the goal of the program is to provide a technology base for industrial exploitation, the program fosters research in industry and energy technology centers, as well as in national laboratories and universities. The program serves as the focus for the transfer of research results to the private sector.

HS&T Hearing
Energy Development
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Asked by Don Fuqua
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General

QUESTION: The fossil energy budget anticipates certain actions by the Congress and has planned to reduce manpower accordingly. In the event that the Congress provided more funds than requested in the FY 1983 budget, what action would DOE take to assure that adequate manpower is available to administer those programs properly?

ANSWER: In the event that final FY 1983 Congressional action on the Fossil Energy R&D budget provided additional funds, Fossil Energy would reevaluate its manpower requirements in light of these budget increases and take the necessary action to insure that adequate manpower is available to administer the programs. The Assistant Secretary for Fossil Energy has already made extensive reviews of both headquarters and field organizations and the staffing levels and skill mix required to support Fossil Energy programs. If significant changes were to occur in regard to Fossil Energy's FY 1983 budget they would be reassessed against our organizational structure and manpower levels to insure that a proper complement of federal employees are engaged in the conduct of the program.

HS&T Hearing
Energy Development
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Asked by Don Fuqua
For Hearing 3/18/82

General

QUESTION: All pilot plant activities are being stopped prior to FY 1983. What are the contract ramifications for these cancellations? What alternatives are being pursued by DOE in the disposition of these facilities? EDS. H-Coal. Bi-Gas. Wilsonville. Ft. Lewis. Wood Ridge. CE. Hygas. Westinghouse. MHD's CDIF and CFFF. Why is the Mobil MTG German facility work being continued? Provide plans and status for each of these installations.

ANSWER: The ramification of contract cancellations of pilot plant activities varies with each project and cannot be accurately summarized in a general statement due to the differences in the terms of the various contracts and agreements. However, the ramification is addressed for the major facilities in the following discussions of alternatives being pursued, current plans and the status of the disposition of these facilities.

EDS (Baytown, Texas): The ramification of terminating DOE participation in the project prior to the conclusion of Phase V (operations) would have been that the royalty right entitlement would revert to those rights accrued as of the completion of Phase IV (construction). Upon withdrawing from the project, DOE would have been required to return to Exxon Research and Engineering all data delivered to DOE with limited rights.

However, DOE successfully negotiated with the Sponsors Management Committee during the summer and fall of 1981 to rescope Phase V to compensate for the shortfall in the Government's share of funding in FY 1982. Essentially, the Government will contribute to the second coal test and ongoing support R&D to the extent that funds are available. The industrial participants will totally fund the originally planned third coal test and the balance required for ongoing R&D support. In return, they will receive a greater share of royalty right entitlement commensurate with their increased contribution towards the total project cost and the Government's share will be reduced accordingly.

The third coal test is estimated to be completed in October, 1982, at which time the dismantlement of the plant and restoration of the site to its original condition would be initiated unless Exxon approaches DOE with an alternative proposal by the spring of 1982. DOE's share of plant disposition costs are held in reserve in the obligated operating expense funds.

H-Coal (Catlettsburgh, Kentucky): The prime contract between DOE and Ashland Synthetic Fuels, Inc. (ASFI) provides that should a participant desire to continue operation of the pilot plant, ASFI will negotiate in good faith to reach mutual agreement on the terms and conditions for a program of continued operation. Negotiation for continued operation must include transfer of title of the pilot plant and benefits to compensate for net depreciated value on a cost basis of equipment and personal property.

ASFI has formally notified DOE that "Ashland is prepared to negotiate a modification to the contract which would waive the obligation (i.e., plant demolition) of DOE in return for transfer of title to the plant from the DOE to Ashland." Negotiations are currently underway and are progressing. However, the potential outcome of the negotiations is still uncertain.

If negotiations are successful, phase-down and demolition funds in the amount of approximately $23.9 million that are being held in reserve, would be used to continue plant operations to generate additional data required to reduce the risks associated with the scale up of the technology.

Additionally, the funds would provide continuity during the transition from government to private funding. This approach has been discussed with and approved by the H-Coal Management Advisory Committee. Changes would need to be recycled back through that committee.

In the event negotiations are not successful, the Government is obligated to return the plant site to its original condition. This would include the cost of phasing out the workforce as well as dismantling the plant and restoring the site.

BIGAS (Homer City, Pennsylvania): There are no adverse contractual ramifications in the termination of the BIGAS project. The pilot plant has been operated since 1976 to establish the technical parameters essential to the definition of the process. It is believed that all the development objectives for this technology will be achieved during FY 1982 and the gasifier concept will be ready for transfer to industry for any subsequent development and/or use.

At present, there is no private sector involvement in the development of this process. It should be noted, however, that Koppers Babcock

Wilcox Corporation (KBW) of Pittsburgh has indicated a possible interest in the concept of conducting development work on their gasifier at the BIGAS site. To date, the interest has been limited to general discussions.

The BIGAS concept has advanced well beyond the stage that the DOE can justify continued participation in any further development because any subsequent use of the facility would involve test programs with near-term objectives and/or at a scale more suited for commercialization than long-range R&D. Accordingly, it is being recommended that the BIGAS pilot plant program be terminated and the facility be mothballed until a decision is reached on how to best dispose of the associated government property.

Wilsonville (Wilsonville, Alabama): No new funding is provided in FY 1983 for the Wilsonville liquefaction facility, a 6 ton-per-day unit owned and operated by the Electric Power Research Institute (EPRI) and a subsidiary of the Southern Company. Sufficient funds, however, are expected to be available from FY 1982, to continue long-range liquefaction research at the facility. DOE funds will be supplemented by funding from EPRI during 1983.

Ft. Lewis (Tacoma, Washington): The disposition problems associated with the termination of Ft. Lewis have been for the most part resolved. Decontamination efforts have been completed and arrangements have been made to dispose of excess government property. The Army has requested that warehouse, storage and administrative type buildings remain intact and it is anticipated that a RFP to dismantle and cleanup the plant will be initiated in the third quarter of FY 1982. Two ramifications of project termination is a disputed foregone fee of $1,866,790 claimed by the prime contractor (Pittsburg and Midway Coal Mining Company); and an obligation on the part of the government to continue environmental and safety monitoring for approximately two more years.

Wood Ridge (Wood Ridge, New Jersey): If the provisions of the contract for the Curtiss-Wright project are strictly interpretated, there are no termination costs to the government. However, if the contractor appeals this interpretation, potential issues to negotiate would include reimbursement for total energy system, site restoration, engineering costs and vendor commitments.

DOE is presently attempting to renegotiate an agreement with Curtiss-Wright to increase industry's share of the cost and to cap government funding. On March 3, a proposal was submitted by Curtiss-Wright which DOE is currently reviewing. The ultimate disposition of this facility will be determined by the final outcome of the negotiations.

Combustion Engineering (Windsor, Connecticut): The atmospheric entrained-bed gasifier has been operated since 1977 and by mid 1982, all technical objectives of the project will have been achieved. Since the project has advanced to the point whereby scale-up of the concept should be the responsibility of the private sector, and since the low-Btu product is a low priority fuel on the clean alternative fuels spectrum, no alternatives to terminating the project are being considered. In addition, the project will have reached the originally planned conclusion and as a consequence, no adverse contract ramifications are anticipated. Currently, DOE is proceeding to modify the contract to include mothballing activities.

HYGAS: The HYGAS pilot plant was successfully operated from 1971 to July 15, 1980, to generate data to optimize the HYGAS process design and to support a conceptual design effort of a commercial demonstration plant (completed in April 1979). In FY 1981, the facility was modified to gasify peats of various moisture content to compile a data base for peat gasification. Four successful peat gasification tests have been conducted to date and the final test is expected to be completed in mid-March. With the successful completion of the fifth test, it is believed that all of the objectives of the peat gasification project will be achieved and the risks of continued development and/or commercialization are low enough for the private sector to undertake.

Development of the HYGAS process and the peat gasification project have advanced to their logical conclusion and, as a consequence, no adverse ramifications are anticipated from the 'mothballing' of the facility. IGT has indicated interest in the facility for transfer to the Institute, and this will be explored. The only other alternative that may be considered is to dismantle the facility.

Westinghouse (Waltz Mill, Pennsylvania): The agglomerating fluidized-bed development effort at the Westinghouse facility has been incrementally funded since 1975 and is being concluded at a logical milestone as a near-term technology. No adverse ramifications are expected and the alternative to 'mothballing' or transferring the facility under suitable terms to Westinghouse that is being considered is dismantlement of the gasifier and associated components.

MHD's CDIF and CFFF (Butte, Montana and Tullahoma, Tennessee): The MHD pilot plants differ from other pilot plant projects in that they are not co-funded. As such, the CFFF and the CDIF are DOE facilities on DOE leased soil, and the termination of these contracts during FY 1982 would not result in any adverse ramifications.

The potential use of the facilities in support of continuing fossil energy and other government programs is being evaluated. However, should an alternative use of these facilities not prove feasible, it will be necessary to close down and dispose of them. Methods of facility disposal are presently under review by the Department.

MOBIL MTG Facility (Wesseling, Federal Republic of Germany): The Mobil methanol to gas work at the German facility is being continued because of the government-to-government agreement, the almost-completed stage of the project, and the high probability that royalty payments will exceed the Government investment in the project if it is completed. In addition, data and licensing provisions in the agreement assure that the results of the pilot plant effort will be available to U.S. industry and the data on the design and operation of the facility as well as the conceptual design study of a commercial plant will be provided to the Department. Third party licensing is also permitted on reasonable terms and conditions so that interested U.S. firms can utilize the technology if the success of the program merits such use.

HS&T Hearing
Energy Development
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Asked by Don Fuqua
For Hearing 3/18/82

General

QUESTION: Every program is characterized by significant decreases in funding from FY 1982 to requested FY 1983. What is the general policy on carrying work forward on FY 1983 funds versus contract cancellation charges? What amount of the total FY 1983 request will be used to cancel contracts in FY 1983?

ANSWER: The FY 1983 Fossil Energy budget request of $107 million provides only for a program of those continuing activities in the area of generic and technology base research. No funds are included in the FY 1983 amount for contract cancellation charges related to the termination of activities no longer consistent with the FY 1983 programmatic objectives. In accordance with this policy, we propose to begin the termination in FY 1982 of selected activities related to those discontinued efforts and intend to utilize available FY 1982 appropriations for payment of termination costs. A deferral of $45 million of FY 1982 appropriations would finance the consolidation of the Fossil Energy Headquarters and field organizations.

HS&T Hearing
Energy Development
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Asked by Don Fuqua
For Hearing 3/18/82

General

QUESTION: Fossil energy facilities are extensive. These assets include not only the Energy Technology Centers, but also government-owned equipment and facilities operated by others. Please provide an estimate on replacement cost of the five Energy Technology Centers. Information is also necessary on the extent and locations of major equipment and facilities elsewhere. In view of the extent of these facilities, why is there no request for any preventive or corrective maintenance on any buildings or equipment for which fossil energy is responsible?

ANSWER: A preliminary estimate of the replacement value for the five Energy Technology Centers (ETC's) and two Mining Centers is approximately $500 million. Each Center's replacement cost are summarized in Attachment A. If desired, a more detailed breakout will be made available. These costs include major plant and equipment at field test sites as well as at the Center.

Information on major equipment and facilities elsewhere is construed to mean pilot plants (PP) and process development units (PDU). A reasonable estimate of the replacement value of pilot plants and PDU's cannot be developed since several facilities were constructed in the mid 1970's and determining a replacement value of the specialized equipment contained in the facilities would border on a conceptual cost effort.

Funding for preventive and corrective maintenance of buildings and equipment at the ETC's is requested under the category of General Plant Projects within the activity of AR&TD. The amount requested for FY 1983 is $1,000,000. An improvement to an ETC that is estimated to cost in excess of $1.0 million is assigned a project number and is separately identified within AR&TD. The total funding for general plant projects and improvements to the ETC's from FY 1977 through FY 1982 is $41,380,000.

Attachment A

ESTIMATED ENERGY TECHNOLOGY CENTER REPLACEMENT COSTS
(Land, Buildings, Utilities, and Equipment)

	$ millions
BETC	$ 48.6
GFETC	32.4
LETC[1/]	90.9
METC	109.4
PETC[2/]	227.5
TOTAL	$508.8

[1/] Includes $50M for Anvil Points.
[2/] Includes $73M for the Mining Technology Centers.

HS&T Hearing
Energy Development
& Applications Sub.
Asked by Don Fuqua
For Hearing 3/18/82

General

QUESTION: The fossil energy budget request claims continuing support for environmentally related research and there is repeated reference throughout the document to work on environment and environmental control devices. What central planning and coordination exists in DOE to assure that all these projects in Fossil Energy and in other parts of DOE are comprehensive in scope, excluding nothing of importance and avoiding duplication? Similarly, what coordination exists between DOE and EPA?

ANSWER: Fossil continues to recognize the importance of the environment in the development and conduct of its technology programs and considers it to be as important as the technical and cost aspects of the program. Coordination and planning of the environmental work associated with our program occur on several levels both with DOE and with other Federal agencies. Major coordination and planning are in the areas of 1) research and development programs, 2) environmental policy, and 3) compliance.

The bulk of the funding for environmental aspects of our program is associated with the implementation of the research and development program.

Intra-Agency Coordination

The environmental coordination and planning for the implementation of our program occurs within DOE and with other Federal agencies. Within DOE there is a close working relationship, which has developed over several years, among those organizations with environmental R&D responsibilities. Several years ago, various components of DOE with environmental responsibilities formally identified the major environmental concerns associated with FE technology development and published this information in Environmental Development Plans (EDP) in years 1978, 1979, and 1980.

With agreement on the major environmental concerns established, the implementation of specific R&D programs to address environmental concerns was initiated through joint programs with DOE organizations having environmental research and development responsibilities. Intra-agency cooperative R&D efforts were and continue to be primarily between the Office of Health and Environmental Research and the Office of Fossil Energy. These efforts resulted in jointly funded complementary environmental programs in the liquefaction, gasification, combustion and oil shale programs. Though the FE program has been redirected, intra-agency cooperative R&D efforts are continuing at the appropriate level for the anticipated program.

Inter-Agency Coordination

Planning and coordination of DOE environmental R&D with EPA has been ongoing for several years. In addition to informal coordination at the staff level, there is a formal DOE/EPA memorandum of understanding in which DOE and EPA agree to share information from DOE facilities, allow EPA access to DOE facilities, etc. Under this agreement, DOE/FE provided EPA with process design information from our SRC-I, SRC-II, Exxon Donor Solvent, and H-Coal projects. Also, DOE provided detailed technical reviews of the Draft EPA Pollution Control Guidance Documents, e.g., gasification, liquefaction, oil shale. There is also a memorandum of understanding between Fossil Energy and EPA's Office of Research and Development which focuses specifically on coordination and planning for environmental control technology R&D for sulfur oxides, nitrogen oxides, particulates, waste management and systems optimization for industrial and electric utility sources.

The two agencies have jointly funded projects, and FE has transferred funds to EPA to conduct research in this area. The two groups meet at least semi-annually and the last such meeting occurred in January 1982.

DOE and EPA also coordirate on the acid rain issue through the Inter-agency Task Force on acid precipitation.

For the coordination of environmental policy, we have a small central environmental staff which coordinates policy aspects of our enviromental program and serves as the focal point in Fossil Energy for interacting with other parts of DOE and other Federal agencies. For example, this group has the lead role for my office in our acid rain activities and assembling our information on the potential impact of proposed legislation and regulations on FE technology development. This group serves on the established intra and inter-agency committees for these subjects.

For the coordination and planning of enviromental compliance activities, we have delegated that responsibility to the line field and/or project managers responsible for implementation of our programs. The interaction with the proper authorities within DOE and outside DOE is mandatory. In addition, the headquarters staff has the function of environmental compliance overview which includes the monitoring of our progress regarding overall compliance matters.

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Asked by Don Fuqua
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General

QUESTION: What colleges and universities are involved in the following programs:
a. Advanced Research and Technology Development
b. Mangetohydrodynamics (MHD)
c. Enhanced Oil Recovery
d. Oil Shale

ANSWER: Attached are lists of the colleges and universities, along with pertinent project information, that are involved in the subject programs. It should be noted that the University Coal Research program grantees are not included as part of the AR&TD program.

Advanced Research and Technology Development

U. of Cal., Irvine
Stanford U.
Yale U.
Mass. Inst. Tech.
U. of Mich.
Princeton U.
U. of North Dakota
State U. of N.Y. (Buffalo)
New York U.
Syracuse U.
Carnegie-Mellon U.
Penn. State U.
East Tenn. State U.
Brigham Young U.
U. of Virginia
West Virginia U.
U. of Cal., Santa Barbara
Col. Sch. of Mines
U. of Illinois
Behrend College
Va. Poly. Inst.
Vanderbilt U.
Cal. Inst. of Tech.
Kansas State U.
U. of Mass.
Clark U.
U. of New Mexico
City College of N.Y.
Clarkson Col. of Tech.
Case Western Reserve U.
U. of Houston
U. of Utah
Auburn U.
U. of Cal., Berkely
U. of So. Cal.
U. of Delaware
Purdue U.
Montana St. U.
Worchester Polytech. Inst.
State U. of N.Y. (Binghamton)
Okla. State U.
U. of Pittsburgh
Duquesne U,
U. of Tenn.
Rice U.
Va. Commonwealth U.
U. of Wyoming
So. Ill. U.
Ill. Inst. of Tech.
Geo. Washington U.
New Mexico Inst. of Mining & Tech.

Magnetohydrodynamics

Miss. St. U.
U. of Tenn.
Mass. Inst. of Tech.
Stanford U.
Polytech. Inst. of Brooklyn

Enhanced Oil Recovery

Leland Stanford Junior U.
U. of So. Cal.
U. of Delaware
U. of Florida
Ill. Inst. of Tech.
Northwestern U.
U. of Kansas
U. of Minn.
U. of So. Miss.
New Mexico Inst. of Mining & Tech.
New Mexico St. U.
St. U. of N.Y.
Columbia U.
U. of Okla.
Okla. St. U.
Lehigh U.
Carnegie-Mellon U.
Penn St. U.
U. of Texas
Texas A&M
U. of Houston
Texas Tech. U.
West Virginia U.
Louisana St. U.
Col. St. U.
U. of Ariz.
Va. Poly. Inst.
U. of Wyoming

Oil Shale

Col. St. U.
U. of Utah
U. of Wyoming
Wash. St. U.
U. of St. Louis
New Mexico St. U.
New Mexico Inst. of Mining & Tech.

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March 5, 1982

Hon. Don Fuqua
House Committee on Science & Technology
Rayburn House Office Building
Room 2269
Washington, D. C. 20515

Dear Congressman Fuqua:

I have been made aware of the upcoming hearings by your committee to review the Department of Energy budget for the fossil energy program for 1983.

As Vice President of Research & Development of a major U. S. coal company as well as a former ERDA and DOE employee, I wish to include in your testimony and for the record the following comments. I was hoping that I would be able to personally participate but it looks like my schedule will not allow me to do so at this time. However, I will be glad to participate in future hearings to provide a coal industry viewpoint to the current budget reductions.

First of all, I was born in Beckley, West Virginia, the son of a coal miner and from a family that has had its roots in the coal mining industry for nearly eight decades. I attended Virginia Polytechnic Institute in Blacksburg, Virginia, on a coal mining scholarship, studied chemical engineering and after nearly twenty years of working in the chemical and government environment, returned to become the Vice President of Research & Development for United Coal Company in April, 1979.

During these past three years, I have dedicated myself, as well as the people at our company, to improving technology of our coal industry and implementing many of the innovations and developments that have been achieved as a result of federal, DOE and Bureau of Mines' programs. Consequently, we have had, during this period of time, several important programs that were co-sponsored by the Department of Energy and the Bureau of Mines with funding also contributed by our company. These programs have been extremely beneficial in that by a coal company participating directly with the federal government, the technology transfer is greatly enhanced. We as a coal company have been extremely selective in identifying those projects that have commercial value; ones we can benefit from rather than just doing "paper studies."

Upon returning to the coal industry, after some twenty years of working in other industries, I was amazed at the lack of technology that has been developed and implemented within this industry. I believe it's fair to say that the technology associated with the coal industry lags behind the other industries, particularly nuclear and chemical, by some twenty years or more. In my opinion, it is primarily due to the lack of the federal catalyst that is established by support from the federal government in technology development.

Unlike the nuclear industry, the coal industry has received during the past twenty years only a very small percentage of what has been devoted to the nuclear industry in government support. Consequently, the coal industry itself is naive in even the methods of going about receiving federal funds. It is obvious to me that the coal industry above all other industries stands to benefit the most from federal support in the technology area because it is so badly needed. Very few areas of the coal industry exist which cannot be improved.

Unlike the aerospace or nuclear industries where one has to work really hard to try to find new ways to improve the technology which already exists, the coal industry is fertile ground for technology development. However, due to the up and down cycle of the coal industry and the lack of the profit margin on this very capital intensive industry, the coal industry itself has been unable over the past fifty years to contribute much of its profit to research and development of its own industry because the margin of profit has been so small with regard to the cost of just producing coal. This is primarily due to the fact that the coal industry is probably the best example in this country of the free enterprise system working to maintain low prices. The competition that exists because of all the different companies participating in the coal industry has provided the lowest possible price for coal. For this reason there has been absolutely little, if any, margin for excess profit which could be devoted to research and development.

It has been said of the coal industry that "it has been so busy trying to make ends meet that it has been unable to devote additional money to some of the technology advancements that will ultimately improve the efficiency of operations." This is where the federal government during the past four or five years has contributed greatly to the coal industry. There has been more money contributed by the federal government during these past five years than in all the history of federal support to this industry. These funds, although many times misplaced like so many other federal government programs in the area of nuclear and defense, have served as the catalyst to the coal industry to involve this industry in participating and learning from the benefits of technology development. In these past few years we have made great strides in helping to get this coal industry out of the dark ages.

There is still much to be done so it is my opinion that the federal government's support during the past few years has been extremely beneficial in stimulating interest in new technology to improve coal mining; production, coal preparation and coal utilization.

Other coal companies besides United Coal Company were beginning to take an active interest in research and development and are looking at what new technology could do to improve their operations. But now, as they have in the past, with the great curtailment of funds, particularly with regard to coal technology development, the coal industry again finds itself shortchanged in favor of other programs such as nuclear, aerospace and defense. It is the coal industry that needs additional federal support in order to provide this resource to fulfill its most important potential of energy support for this country.

Having worked in the federal government and industry, I have had the firsthand opportunity to witness waste of federal money with regard to all phases of our energy development as well as defense. There has been a magnitude of growth in the "Beltway Bandits" who thrive and live totally off of federal programs. What has been discouraging to me is that these so called "Beltway Bandits" lobby and know the federal system well enough to know whenever the federal government intends to distribute program funds and they are camped at the doorstep to receive those funds and even to influence where the funds should go. However, very little of the technology provided by these types of companies gets back into the coal industry or is transferred to the coal industry in actual application.

This, to me, is the major flaw that exists and has existed in the fossil energy program. Consequently, by having companies like ours participate directly with regard to federal programs, technology is then in the hands of those people who will use that technology rather than going through the TRW's, the Oak Ridge National Laboratory or Los Alamos Scientific Laboratory or Argonne National Laboratory. Such laboratories live and breathe off of the federal dollar. Consequently, they are not necessarily motivated by the purpose of transferring technology development into the industry application but rather motivated by the purpose of development for development's sake.

We at United Coal Company have made a commitment to improving our industry and improving our operations by becoming more efficient and more technology oriented. Consequently, we are picking up on many of the projects and evaluating the commercial viability of those projects that were initiated by the Department of Energy. Such programs as fluidized-bed combustion are now being evaluated by our company with regard to converting the low-grade waste which is now disposed of to make our own electricity and steam. There is much work that needs to be done in taking this low-grade resource and determining its commercial viability but there are no funds available from the Department of Energy to support this work. For example, millions and millions of tons of coal waste exist and are disposed of which contain on the order of 4,000-6,000 Btu/lb.; a heat value that is greater in many cases than the lignite coal reserves of the Western states. However, I have been unsuccessful in obtaining support for such a program primarily because funds do not exist. Another major area of paramount importance to the future utilization of coal is in coal preparation. How do you remove the sulfur at the mine site before it has been shipped to a utility? How do you remove the ash to a lower level so that it does not cause fouling and slagging of boilers? How do you reduce impurities so that they do not have to be shipped and then pay the extra cost of shipping ash and waste products? These are major areas which the Department of Energy identified but as a result of reduced funds are no longer able to support. Personally, I believe it's on the verge of criminal when I look at the funds being contributed to the nuclear program in comparison to the drastic reductions now being proposed for the fossil program.

Do you realize that coal provides 19%-20% of the energy used in this country - and it could provide tremendously more whereas nuclear provides on the order of 6%-7% of our electric needs only. You cannot make gasoline from nuclear power. But the proportional percent of our federal development funds going into the nuclear program are many, many, many times greater than the coal program. And one doesn't even know how much because much of it is hidden under the cloak of secrecy and classified defense related work.

I much prefer to talk about coal research and development rather than fossil research and development because I do not believe we need to have much additional work in the area of oil enhanced recovery. God knows, the oil companies themselves have tremendous profits they can devote to technology development. However, the coal industry is in drastic need of additional federal support if this country ever expects to be able to achieve the reliability and the security based upon our own energy resources.

In closing I would like to make one significant observation to this committee. Coal has the capability to provide an energy resource to both our major energy needs - electricity and gasoline. There are lots of ways to make electricity. However, to make the gasoline we need to maintain a strong defense and maintain our economic growth, coal is that one resource that can do so. When we talk about great budget deficits, one has to only look at the amount of dollars we exported to the Mideastern countries for imported oil last year and you can find that deficit: It is five times more than our current projected budget deficit. For example, last year we paid out of this country's economy nearly $80 billion for imported oil. Very little of that money came back to this country. However, if that money was being spent to make our own gasoline and other petroleum products needed from coal, oil shale and other natural resources we have available to us, that money would circulate within our own economy four to five times. That amounts to approximately $300 billion-$400 billion of value by turning this great exporting of dollars to the Mideast back into our own economy.

It appears simple. The one thing this country needs most to get ourselves out of this spiraling budget deficit is the ability to make our own energy needs within our own continental boundary. We talk about inflation, interest and other economic woes. These are only symptoms. If we start building the plants necessary to convert coal into a liquid fuel to meet, not our electricity needs but our mobile needs such as gasoline, then we stimulate the economy by creating demands for steel, housing, and employment by which each person contributes back into the country both products and tax revenues.

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The coal industry desperately needs federal support for research and development technology. This industry needs to catch up. It is not in a profitable financial situation that will enable it to carry on with research and development because it is too busy keeping the creditors away from the door to spend additional money on long-term improvements such as those demanded by research and development. "If you are knee deep in alligators, you don't have time to figure out a way to drain the pond." Therefore, this is one industry that I don't believe is in a position to greatly carry on technology development without the federal catalyst.

Therefore, I submit to this committee that I will be glad to come and testify just as long as someone will listen to the needs of the coal industry for research and development support. I urge this committee to take a hard look at the energy balances that are now before you with regard to the amount of funding being devoted to support these coal technologies and ask this Administration to re-evaluate if some of the additional dollars being spent for research and development technology with regard to nuclear energy could be diverted back into the coal program. Example after example can be quoted from this budget that can clearly show where dollars can be transferred into support for meaningful and beneficial programs to improve coal mining, coal beneficiation and cleaning and coal utilization.

Sincerely,

Richard A. Wolfe

Richard A. Wolfe, Ph. D.
Vice President
Research & Development

RAW:bs

FISCAL YEAR 1983 DEPARTMENT OF ENERGY BUDGET REVIEW

(Fossil Energy and Basic Research)

TUESDAY, MARCH 23, 1982

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
Washington, D.C.

The subcommittee met, pursuant to notice, at 9:35 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman) presiding.

Present: Representatives Fuqua, Gore, Heckler, Winn, Volkmer, Wolpe, Fish, Sensenbrenner, Skeen, Schneider, Dunn, and Walker.

Mr. **Fuqua**. The subcommittee will be in order.

We continue our review of the fiscal year 1983 DOE fossil energy R. & D. programs. Today and tomorrow we will receive testimony from outside witnesses representing organizations with interests in fossil energy R. & D.

The DOE intends to redirect the Federal effort toward long-term, high-risk, technology base research. Under this concept, technology development, beyond proof of concept, will be left to the private sector. We have been told that sufficient incentives arising from oil price decontrol, the creation of the Synthetic Fuels Corporation, and provisions of the new Tax Act will initiate private sector R. & D. in this area.

Therefore, we will be interested in the opinions of the private sector regarding the proper public and private roles in research, development, and demonstration of new technologies. The subcommittee would also appreciate the views of industrial representatives regarding the future of current programs faced with proposed funding reductions and terminations. Do the market incentives exist to induce further private sector involvement in these programs?

Members of our first panel are: Mr. Kurt Yeager and Mr. Dwain Spencer, division directors of the Electric Power Research Institute; Dr. Susan Wu, the administrator of the University of Tennessee Space Institute; and Mr. Dave Brown, the president of MHD Industrial Forum. If those witnesses will please come forward, we will proceed.

As we usually do, we will go through and hear your testimony and then we will have questions because many of the questions may have an interest to one or more of you, and if you have prepared statements, we will make them in their entirety part of the record. If you choose to summarize, we will be happy to have you do that.

Mr. Yeager.

STATEMENT OF KURT E. YEAGER, DIRECTOR, COAL COMBUSTION SYSTEMS DIVISION, ELECTRIC POWER RESEARCH INSTITUTE

Mr. YEAGER. Thank you, Mr. Chairman, members of the subcommittee.

I am Kurt Yeager, director of the coal combustion systems division of the Electric Power Research Institute, and I have prepared a formal statement which I would be pleased to provide for the record. I will summarize a few key points out of that in the time I have this morning.

Mr. FUQUA. It will be made part of the record.

Mr. YEAGER. Thank you.

My testimony today will focus on joint and coordinated EPRI/Department of Energy activities and the effects of Federal funding reductions and termination on the development of direct coal combustion technology.

Coal is by far the most abundant domestic fossil fuel resource and has been the cornerstone of the national goal to achieve increased energy self-sufficiency under at least the last four administrations. Coal is expected to grow to provide more than 40 percent of the energy needs of the Nation by the end of this century. In meeting this demand, the vast majority of this coal will be utilized in direct combustion by electric utilities.

The Synthetic Fuels Corporation has been established as the primary Federal entity charged with assisting the demonstration and commercialization of synfuel technologies. Unfortunately, no such mechanism, designed to avoid the pitfalls of the last 10 years, has been provided for direct combustion technology. As a result, opportunities in coal cleaning, fluid-bed combustion, and other advanced combustion technologies, as well as environmental control technology, all of which have a very large impact on the quantity and quality of coal utilization over the next 20 to 30 years are being constrained from successful and timely development and commercialization.

My comments on the fiscal year 1983 budget request for fossil energy research and development will focus on three major budget categories reflecting the DOE efforts in coal combustion-related research and development. These are control technology and coal preparation, advanced research and technology development, and combustion systems.

The 1983 request for these three categories totals less than $35 million, over a 70-percent reduction from the 1983 base of more than $122 million.

First, with respect to coal preparation and analysis, coal quality is central to the efficiency, reliability, and environmental performance of coal combustion, the production of coal slurries, and the preparation of feedstock for coal conversion processes. Unfortunately, Department of Energy support for this area has been and continues to be minimal. This is particularly unfortunate because the opportunities for technological progress are very large relative the investment required. My testimony of last December elaborated on the opportunities in that area.

In addition, such progress can be rapidly achieved and translated into substantial improvements in the quality and quantity of coal utilitization. The current funding request by DOE of $2 million is ineffective relative the needs of the area and should be increased by a factor of 10 to 100 to have measurable impact in this area.

Second, flue gas cleanup: Several years ago Federal leadership for the development and evaluation of flue gas cleanup systems was transferred from the Environmental Protection Agency to DOE. This was applauded as a progressive step toward promotion of resolution of reliability, performance, and byproduct issues that limit the effectiveness of mandated control technology, particularly flue gas desulfurization.

The need here is punctuated by the fact that over 40 percent of the cost of a new coal-fired powerplant is typically for compliance with environmental requirements. Unfortunately, very few little substantive progress has been made with the very limited Government participation in this area.

The operational limitations associated with required flue gas cleanup technology, together with the uncertainty over the future course of environmental regulatory requirements, cause the greatest impediment to the increased use of coal. It is, therefore, disturbing that the Government has maintained a position which effectively discourages cooperation with the private sector in the solution of technical issues created by the very technology it has mandated.

Effectiveness of any Government funding in this category will depend, however, on a much more aggressive R. & D. policy which focuses on the key problem areas, and commits jointly with the private sector to the major technology projects required for their resolution. I might note, by comparison, that the Electric Power Research Institute is investing in this year alone—five times the amount of the 1983 DOE request of $3 million.

In areas of advanced research and technology there are five programs of special importance to the direct combustion of coal. These are direct utilization, materials, engineering analysis, environmental activities, and university coal research. This combined area is proposed to receive a 64-percent budget reduction to $10 million in 1983, including elimination of the university coal research program.

I will touch on just one topic in this area and that is materials research. Government support and facilities for the research, development, and validation of new and improved materials has been fundamental to the technological leadership that this Nation has enjoyed. In my judgment there is no more cost-effective application of Federal R. & D. support, which is also entirely consistent with the new Federal policy of long-range/high-risk research support.

The vigorous environment in which coal combustion and conversion technologies must operate, as well as the need for improved performance in our existing powerplants, is placing drastic new demands on our materials science and engineering base. Funding for this program should be substantially accelerated, not reduced.

Turning now to the third area, the combustion systems program, which is that which deals primarily with the commercialization of advanced combustion technology.

First, the atmospheric fluid bed combustion program is an example of the success that can be achieved through a well planned and consistent Government research, development, and demonstration program conducted in a manner complementing private sector capabilities and objectives. Based on the progress of this effort, focusing on industrial applications, the Electric Power Research Institute and the utility industry are pursuing the application of atmospheric fluid bed combustion as a further evolutionary improvement in coal utilization to meet the new requirements of today. EPRI investments in this area will total over $80 million.

TVA has been the lead Federal agency in this area, and they have constructed now the key engineering prototype facility for the engineering confirmation of this technology in a utility context. This facility, I would underscore, has only been possible with Federal R. & D. support through TVA. The results will provide a technical basis for a 100- to 200-megawatt demonstration to begin construction, we hope, in 1984. Unfortunately, further Federal funding through TVA for the construction of such a facility has been canceled. This leaves the funding mechanism for the final R. & D. step uncertain. In the absence of further Federal R. & D. support EPRI and the utility industry hope to develop a private-sector funding consortium for the construction of these major pioneer facilities.

Providing the necessary confidence in such a step, however, puts a major burden on the technology development effort. If the private sector is to be responsible then the demonstration of this and any other technology becomes less the last step in the development process and more an assured commercial venture. Fortunately, in the case of atmospheric fluidized bed combustion the continuity and success of the previous Federal/private combined effort may make this demonstration possible.

I would emphasize, however, that it is likely to be the exception rather than the rule unless Federal support for high-risk full combustion research and development, including the critical pilot facilities, is maintained at a substantially greater level than requested in 1983.

The last topic I would comment on is perhaps the most important of all—pressurized fluidized bed combustion. The impact of DOE funding reduction is nowhere more immediately evident than in this particular development program. Major progress has been made over the last several years in converting this technology from what was at the time an engineering curiosity to what is now an important electric power producing technology, having among the lowest bus bar energy cost potential of any of our near and midterm coal utilization options. The effective cancellation of DOE support on the brink of successful proof of commercial viability is indeed seizing defeat from the jaws of victory.

I am particularly concerned about two key test facilities in this area which have been developed and funded with DOE support, specifically the Curtiss-Wright and the Grimethorpe facilities. They are the key instruments for achieving the necessary confidence to translate this technology into commercial application. In considering the merits of these facilities it is important to recognize that they are technically complementary and an either/or funding mentality is very counterproductive. In my judgment no single issue is more critical to the success of pressurized fluidized bed technology than maintaining these facilities during the current phasedown of Department of Energy support.

In this regard I am especially concerned about the Curtiss-Wright facility. In any analysis of the path by which the PFB combined cycle reliability issue must be resolved the large pressurized fluidized bed gas turbine pilot under construction by Curtiss-Wright is a key step. My concern is based on the lack of requested funding for this project in fiscal year 1983 and the very limited release by the Department of Energy of the fiscal year 1982 funding for the completion of construction of this facility.

Based on our judgment that the project is critical to the development of pressurized fluidized bed combustion, EPRI has offered to work with the Department of Energy to provide a practical transition to private sector support. In this regard we are prepared to cooperate in the R. & D. operation and testing phase of this project; however, such participation is contingent on completion of facility construction as originally planned by the Department of Energy and Curtiss-Wright.

In conclusion, the actions of the Federal Government in direct support of energy-related research and development, and in the form of incentives, have a profound effect on the utility industry's ability to develop and use new coal utilization technology. Federal R. & D. policy with respect to the utility industry needs to understand and consider the underlying circumstances affecting this industry and the importance of electric energy to the Nation's energy, security, and economic needs. Continued Federal support to share the risk in selective coal combustion programs will be necessary if commercialization of these improvements is to be achieved.

Research and development groups are fragile entities that once disbanded cannot be restored. I would, therefore, urge that whatever changes may emerge from the new Federal energy R. & D. policy be made gradually so that the system can adjust with minimal damage. Unfortunately, the reductions proposed in the 1983 Department of Energy budget have occurred so suddenly the opportunity for coordination to reduce the shock has been minimal in most programs. The reduction of over 70 percent to just $35 million in 1 year for direct combustion-related research and development should be increased to at least $85 million in 1983 to achieve a truly effective transition to private sector support. This will still represent a major reduction in the Federal participation from that indicated in the 1983 base.

There is no shortage of opportunities to improve the efficiency and environmental performance of coal combustion. The successful development of our joint objectives in these areas, however, require a much more constructive partnership between Government and the private sector to conduct coal combustion research and development and to implement the results. EPRI is prepared to do its part in both formulating and funding these efforts.

Thank you very much.

[The prepared statement of Mr. Yeager follows:]

STATEMENT OF

KURT E. YEAGER
DIRECTOR, COAL COMBUSTION SYSTEMS DIVISION
ELECTRIC POWER RESEARCH INSTITUTE

BEFORE THE
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
COMMITTEE ON SCIENCE AND TECHNOLOGY
U. S. HOUSE OF REPRESENTATIVE

MARCH 23, 1982

INTRODUCTORY STATEMENT

Mr. Chairman, Members of the Subcommittee:

I am Kurt E. Yeager, Director of the Coal Combustion Systems Division of the Electric Power Research Institute (EPRI). I appreciate this invitation to again meet with you to discuss the effect of changes in the Federal program of energy research and development. These changes have significant implications for the electric utility industry and therefore are of great importance to EPRI.

The EPRI was established by the electric utility industry in 1973 to plan and manage comprehensive research development and demonstration program for the entire electric utility and its rate payers. EPRI is supported by voluntary contributions from investor-owned, government-owned and cooperatively-owned electric utilities across the nation. Over the next five years, EPRI will direct about $300 million to improving direct coal combustion systems. These efforts will span coal quality control, powerplant performance and reliability, fluidized bed combustion and environmental control.

In terms of allocation of the EPRI Coal Combustion Systems Division's effort, the largest part of our work (70-80%) is focused on the next five years, providing improvements in the performance and reliability of existing powerplants and environmental control. The remainder of the division's effort is devoted to next generation coal combustion technology, especially fluidized bed combustion and coal slurry fuels.

This approach fits quite naturally with a Federal research and development program which emphasizes longer term developments and more basic research. Since its inception, EPRI has worked with the Department of Energy (and its predecessor agencies), the Environmental Protection Agency, and other Federal agencies with energy research and development responsibilities to achieve mutual technical objectives.

My testimony today will focus on joint and coordinated EPRI/DOE activities, and the effects of Federal funding reduction and termination on the development of direct coal combustion technology. These comments are intended to augment my testimony to the Subcommittee on February 24, 1981, and December 10, 1981 and represent my personal viewpoint on the issues.

The Role of Coal

Coal is by far our most abundant domestic fossil fuel resource and has been the cornerstone of the national goal to achieve increased energy self-sufficiency under each of the last four administrations. The imperative for coal has grown rapidly over this period as the reliability and security of our petroleum supply becomes more uncertain and the initiative for nuclear power is delayed. In addition to providing over 50% of the electricity generated today, coal is expected to provide about two-thirds of the nation's energy growth over the next 20 years. The large majority of this growing coal utilization will be in the form of direct coal combustion, primarily by the electric utility industry.

There are, however, several barriers which continue to impede the realization of coal's potential. These include:

- Deteriorating quality of the coal produced, not only in absolute heating value and combustion behavior, but in the day-to-day variations of these properties.
- Declining thermal efficiency and reliability of conventional powerplants.
- Scarcity of uncommitted water supplies.
- Environmental control requirements.
- Complexity and time required for construction and operating permits.
- As a consequence of all the above, escalating costs.

Resolution of these barriers has been the common goal of both Federal and private sector coal utilization R&D efforts.

The Impact of Federal Coal Utilization R&D

Before discussing the specific effects of reduced Federal support, I would like to make some observations regarding the general impact of the Federal R&D effort over the past ten years on achieving this common goal of increased coal utilization.

Prior to the Arab Oil Embargo of 1973 the nation's efforts in coal R&D were primarily funded and directed by the private sector. The modest Federal program in coal utilization was focused on the Department of Interior's Office of Coal Research and Bureau of Mines. The function of this program was consistent with the historic role of government R&D participation, i.e. to understand the availability and quality of our coal resource and to stimulate long-range, high risk R&D which industry would not be in a position to finance alone.

After the oil embargo, a major increase in the function and scope of government participation in energy R&D quickly evolved. This was based on the proposition that major government cost-sharing was essential through the commercial demonstration phase of development if promising new coal technology was to reach the market place quickly. This requirement for acceleration in turn rested on the government's primary consideration that development of improved coal utilization technology was now a matter of national interest and security. Unfortunately, in responding the government has acted as if it, not the private sector, was the ultimate purchaser and user of the results.

Despite this appreciable growth in Federal support for energy R&D, there are relatively few government-funded projects which have resulted in the commercialization of new technology or have a probability of doing so. This has resulted from differences in application, not lack of opportunity or need. In my judgement, two major factors have restricted the effectiveness of the expanded Federal role.

First, because the benefit to the participants was considered secondary, the government has avoided use of free enterprise incentives to promote technology development. DOE procurement and contract practice has for example, typically utilized competitive solicitations, including patent and data rights provisions which eliminate commercialization incentives, and has insisted on government management of the project. Many promising energy technology developments are highly proprietary and result from years of corporate effort. They mature at different times and thus single competitive solicitations often attract inferior or premature proposals. Patent and data right provisions discourage utilizing government support for highly promising proprietary technology, with the net result that solicitations seldom attract the most advanced projects. Finally, few companies are willing to make significant cost sharing contributions if management responsibility resides solely with the government. The energy systems required to meet our energy needs must be economically viable as well as technically feasible. They must be utilized by a highly diversified and competitive energy industry as opposed to government alone. To be effective government must reinforce not replace the private market forces guiding our energy economy.

The second limit factor has been the lack of continuity in Federal energy R&D commitments. The vigor of Federal efforts to achieve "energy independence" over the past few years, has been largely proportional to the length of the gasoline lines at the time. As a result, Federal energy R&D commitments (if not appropriations) have often fluctuated dramatically in response to the political pressures of the moment.

This has further discouraged using government support for the more promising technology and has left credibility with domestic and international developers and cosponsors in disarray. Perhaps the most alarming aspect of this fluctuation in support is that it has been induced by shifts of less than a 5 percent in world oil supply.

The new Federal energy R&D policy proposes to place responsibility for technology innovation and development in the private sector to the maximum possible extent. In this approach, the DOE would end the program of major energy demonstration and would instead focus on supporting longer-range research and development. The success of this policy depends on economic and regulatory incentives to make technology development program attractive and competitive for the private sector to pursue. It is on this topic, as it applies to direct coal utilization by the utility industry, that I will conclude my general remarks.

Coal is expected to grow to provide more than 40 percent of the energy needs of the nation by the year 2000. In meeting this demand the vast majority of coal will be utilized in direct combustion by electric utilities. The Synthetic Fuels Corporation has been established as the primary Federal entity charged with assisting the demonstration and commercialization of synfuel technologies. Unfortunately no such mechanism, designed to avoid the pitfalls of the past ten years, has been provided for direct combustion technology. As a result, opportunities in coal cleaning, fluidized bed and other coal combustion improvements as well as environmental control technology having a much greater impact on the quantity and quality of coal utilization over the next 20 to 30 years, are constrained from successful and timely development and commercialization.

You may well ask why the private sector does not pick up the total required effort in direct coal combustion research, development and demonstration. The answer must consider the unique aspects of the primary using industry-the electric utilities, and their technology suppliers:

- The electric utility industry is a financially stressed, regulated industry. Rates of return are governed by public utility commissions, and therefore, profits do not necessarily reflect risks.
- Many state utility commissions do not permit R&D or the additional costs associated with implementation of first-of-a-kind technology in the utility rate base.
- Energy tax credits for new technology, permitted other industries, are not available to electric utilities.
- Financial problems facing the industry and its inability to generate capital funds for improvement or expansion makes additional R&D funding even more difficult. As a result, EPRI has also had to reduce the scope of its program and funding at a time when the needs are increasing dramatically.
- Major financial risk is associated with incorporating an innovative technology into an established and complex electric power system so that it is both economical and reliable.
- Utility supplier firms cannot afford the investment in R&D necessary to return a sufficient profit in these times of high interest rates. R&D is only the first step in a project which, if successful, will require orders of magnitude greater expenditures for engineering, manufacturing, marketing and capital before a profit is realized. The possibility of changes in government policies over the period further amplifies the risk.
- Under these cost conditions, and with reduced domestic economic growth, the utility suppliers are turning more and more to serving a less sophisticated third world market with existing technology. In that market improved efficiency and environmental control are lesser considerations in the use of coal. Thus a fundamental change in supplier priority may develop which is not compatible with the utilization objectives for coal in this country.

- Foreign countries, where a more cooperative partnership between government and industry exists are aggressively moving to assume leadership in the development and commercialization of improved direct coal combustion technology.
- Loss of technological leadership may force the utility industry and its suppliers to depend on these foreign sources. Thus the free-market may work on an international level but the U.S. will lose the trade advantages of export rather than importing technology. Other, far-searching implications for supplier confidence and the U.S. international position are self-evident in such a loss of leadership.

Taken together, these circumstances pose a problem of major proportions-a problem which should be of national concern. It becomes critical when it is necessary to build large-scale demonstrations. These are essential elements in proving out the economies and technical aspects of some of the new technologies. It is also often necessary to gain experience with several such large-scale projects before the risks are reduced to the level of broad commercial acceptability. Thus, there are a number of programs in direct coal combustion related technology for which the funding mechanism to achieve commercialization is not evident. Furthermore, adequate continuing efforts in supporting technology, for which government funding also is-being reduced, poses a longer term problem affecting the technological base for our industry.

Specific Budgetary Impacts

My comments on the FY 1983 budget request for Fossil Energy Research and Development will focus on three major budget categories reflecting DOE efforts in direct coal combustion related R&D. These are: (1) Control Technology and Coal Preparation, (2) Advanced Research and Technology Development and (3) Combustion Systems. The FY 1983 request for these three categories totals $34,900,000, a 71 percent reduction from the base of $122,452,000. Such a major reduction cannot avoid causing dislocations even under carefully planned circumstances. Unfortunately this reduction has occurred so suddenly that opportunity for coordination to reduce the shock seems to be minimal in most programs.

A. Control Technology & Coal Preparation

1. Coal preparation and analysis	FY83 Request	Net Change
	$2,000	$-880

As indicated in my testimony of December 10, 1981, on this subject, coal quality is central to the efficiency, reliability and envionmental performance of coal combustion. Coal cleaning to remove sulfur and ash is therefore an increasingly important option for the utility industry and is one of the most underdeveloped areas in coal combustion related technology. Priority objectives for research and development to improve coal cleanability forecasts and technology include:

- Improved sampling and analytical procedures to insure confidence in our reserve quantity and quality estimates.
- Techniques for cleaning lower rank Western coals.
- More efficient and economical fine coal cleaning and recovery techniques.
- More economical chemical cleaning techniques providing thorough sulfur and ash removal.
- Automated cleaning process control and on-line continuous measurement techniques.

DOE support for this area has been, and continues to be, minimal. This is particularly unfortunate because the opportunities for technical progress on each of the above objectives is very large relative to the investment required. In addition, such progress can be rapidly translated into substantial improvements in both the quality and quantity of coal utilization. DOE is also blessed with very capable laboratories and staff to perform these efforts. The current funding request of $2,000,000 is ineffective relative to the needs of the area and should be increased by a factor of 10 to 100 to have measurable impact.

2. Flue Gas Cleanup	FY83 Request	Net Change
	$3,000	$-3,240

Several years ago, Federal leadership for the development and evaluation of flue gas cleanup systems was transferred from EPA to DOE. This was applauded as a progressive step promoting resolution of reliability, performance and by-product issues limiting the effectiveness of mandated control technology, particularly flue gas desulfurzation. This need is punctuated by the fact that 40 percent of the capital cost of a new coal-fired powerplant is typically for compliance with environmental requirements. To date very little substantive progress has been implemented, although many opportunities for flue gas cleanup technology improvement exist and cosponsored projects with utilities and others have been developed for DOE participation.

The operational limitations associated with the required flue gas cleanup technology, together with uncertainty over the future course of environmental regulatory requirements cause the greatest impediment to the increased use of coal. It is therefore disturbing that the government has maintained a position which effectively discourages cooperation with the private sector in the solution of technical issues created by the technology it has mandated.

Recommended areas where concentrated R&D effort is required include:

- Improve the reliability of existing slurry (lime/limestone) scrubbing-systems for sulfur dioxide removal.
- Reduce the quantity and improve the quality of disposable by-product associated with slurry scrubbing systems.
- Demonstrate regenerable flue gas desulfurization systems which minimize the use of expensive reagents, produce marketable by products and rely entirely on coal.
- Develop and demonstrate compact "urban scrubber" flue gas cleanup systems supporting the conversion of existing oil-fired power plants to coal.
- Develop simplified flue gas cleanup systems capable of simultaneous removal of sulfur and nitrogen oxides.

The effectiveness of government funding in this category will depend on a much more aggressive R&D policy which focuses on the key problem areas indicated and commits jointly with the private sector to the major technology projects required for their resolution. By comparison, EPRI alone is investing annually in this area about five times the FY1983 DOE request.

	FY83 Request	Net Change
3. Gas Stream Cleanup	$1,500	$-5,796

The most critical application of gas steam cleanup to the direct combustion of coal is in pressurized fluidized bed combustion (PFB). The technical and commercial success of this technology as an electric power generating system depends on the ability of the gas turbine to operate reliably on the dust laden gas steam produced by the PFB. The recent DOE/EPRI workshop on PFB concluded that "the criteria for acceptance of PFB will always go back to the expectation of reduced cost and reliable performance--and that the biggest uncertainty is turbine blade life". The DOE program, with which EPRI has been actively participating, is the basis for research and development on the new and yet unproven cleanup technology that will be required to achieve a viable PFB electric power generating system. The FY83 DOE funding reduction of 80 percent will eliminate many of the most promising options on which further private sector commitment to PFB depends. This is particularly unfortunate since PFB is one clear example where DOE efforts have been instrumental in developing an important but high risk new coal utilization technology nearly to the point where transfer for private sector commercialization is feasible. To precipitously cancel support now on the major remaining technical problems is to risk negating the PFB investment to date.

	FY83 Request	Net Change
4. Cleanup Base Technology	$400	$-4,880

The virtual elimination of DOE efforts to characterize and develop control for liquid and solid waste effluents from coal utilization systems will leave a major void in the ability of industry to understand and respond effectively to the new, and still rapidly changing, requirements of the Clean Water Act, Toxic Substance Act, and Resource Conservation and Recovery Act. The resulting uncertainty and confusion on the part of current and potential coal users will further impede their commitment to coal. Initiatives should also be pursued to eliminate the economic risks and penalities associated with the recovery of raw materials from coal utilization waste products.

B. Advanced Research and Technology

The Advanced Research and Technology Category includes five programs of importance to the direct combustion of coal. These are Direct Utilization, Materials, Engineering Analysis, Environmental Activities and University Coal Research. The FY1983 DOE request indicates these combined programs will receive a 64 percent budget reduction to $10.1 million, including elimination of the university coal research program. Aside from the major technology research and evelopment implications of such a reduction, it appears inconsistent with the Federal policy of emphasizing long-term, high risk, but potentially high payoff research and evelopment which industry is unable to conduct alone.

The reductions of greatest concern to direct coal combustion are first, Materials. Government support and facilities for the research, development and validation of new and improved materials has been fundamental to the technological leadership this nation has enjoyed. The long term benefits to the nation and the scope of required effort, however, exceed the effort which the private sector alone can justify as a profitable investment. In my judgement, there is no more cost-effective application of Federal R&D support which is also entirely consistent with the new Federal R&D policy. The rigorous environments in which both new coal combustion and conversion technologies must operate, as well as the need for improved performance and reliability from our existing powerplants, is placing drastic new demands on our materials science and engineering base. Funding for this program should be substantially accelerated, not reduced.

A second area of concern is Engineering Analysis. Although the intent of DOE is to transfer these efforts to the specific process development programs they support, it is questionable whether they will be continued in the face of the major cutbacks also experienced by the process programs. Third, is the elimination of University Coal Research as a specific program. Despite the intention to carry this out under direct program activities, it appears certain that a major net reduction will occur. In addition, limiting unversity research to supporting specific programs will necessarily constrain the research opportunities and freedom which ultimately provides the innovative foundation of our national scientific and engineering capability.

	FY 83 Request	Net Change
C. Combustion Systems		
1. Atmospheric Fluidized Bed Combustion	--	$-8,788

Conclusion of DOE support for AFB technology development is occurring at a time when the technology is achieving a commercial foothold for industrial application. The DOE program has been instrumental in achieving this demonstrated capability of AFB to reliably produce steam from coal for small industrial and space heating applications. The program has included installation of several industrial demonstrations and is an example of the success that can be achieved through a well planned and consistent governmen research, development and demonstration program, conducted in a manner complementing private sector capabilities and objectives.

EPRI and the utility industry are pursuing the application of AFB as a further evolutionary improvement in coal utilization to meet the new requirements of today. The improvements which excite interest include reduced sensitivity to fuel quality, thus permitting use of a much broader fuel supply without suffering large losses in efficiency and reliability in a single boiler design. Second, AFB indicates less cost sensitivity to unit size in a period when load growth and siting restrictions may prefer smaller rather than larger furnaces. A third primary advantage of AFB is environmental performance. In AFB both sulfur and nitrogen oxide control is achieved within the combustion process, thus eliminating the need for parasitic flue gas cleaning systems.

Development of AFB has successfully progressed from the process confirmation stage to engineering prototype, making commercial utility scale application a distinct possibility this decade if the follow-on demonstration plant can be constructed. TVA has taken the lead, with EPRI support, in implementing the 20 MW_e engineering prototype which will begin operation this month. This facility has only been possible with major Federal R&D support through TVA.

The results will provide the technical basis for a 100-200 MW_e commercial scale demonstration to begin construction in 1984.

Unfortunately, further Federal R&D funding through TVA for the construction of the necessary demonstration facility has been cancelled. This leaves the funding mechanism for this final R&D step uncertain. In the absence of further Federal R&D support, EPRI and the utility industry hope to develop a private sector funding consortium for the construction of this major pioneer facility. Utility sites and cosponsors have been solicited by EPRI and a number of affirmative responses have been received.

It should be recognized however that such a costly project conducted totally in the private sector must satisfy additional, non-technical constraints not generally present in Federally funded demonstrations. The facility must produce an immediate profit for the participants. In the case of the utility industry this means the demonstration must be a commercial power producing facility capable of reliable operation for at least 30 years, at operating costs less than existing technology.

Providing the necessary confidence in such a step puts a major burden on the technology development effort. If the private sector is to be responsible, then the demonstration becomes less the last step in the development process than an assured commercial venture. The continuity and success of the previous combined Federal/private development effort may make this pioneer demonstration step for AFB commercially viable through private support alone. This is, however, likely to be the exception rather than the rule unless Federal support for high risk coal combustion R&D, including critical pilot plants, is maintained at a substantially greater level than requested in FY 1983.

	FY 83 Request	Net Change
2. Pressurized Fluidized Bed Combustion	$ 4,500	$-20.460

The impact of DOE funding reduction is nowhere more immediately evident than in the PFB development program. Major progress has been made over the past several years in converting this technology from an engineering curiosity to an important electric power producing technology having the lowest bus-bar energy cost potential of any near-term coal utilization option. The effective cancellation of

DOE support on the brink of successful proof of commercial viability is indeed "seizing defeat from the jaws of victory".

PFB offers many potential advantages of merit to the utility industry in addition to lowest bus-bar cost. These include high efficiency, low environmental risk and increased fuel flexibility. In addition PFB combined cycle plants, comprised of barge-or rail-shippable components, could yield additional important financial advantages to utilities during the current period of low load growth, high interest rates and high cost of construction-work-in-progress (CWIP). Thus, confirmation of the PFB advantages has become an important objective for EPRI.

Although development results have been encouraging, much additional progress must be made before confidence is sufficient to warrant commercial application. Current limiting considerations include:

- Unproven gas turbine reliability.
- Unproven hot gas cleanup technology capable of operating well beyond the efficiency of inertial separators.
- Need for demonstrated system availability and operability under utility conditions.

Extrapolation to utility service is not likely until these steps have been successfully piloted.

The Curtiss-Wright and Grimethorpe pilot facilities, funded by DOE, are key instruments for achieving this confidence. In considering the merits of these facilities, it is important to recognize that they are technically complementary and an "either/or" funding mentality is counterproductive. In my judgement, no single issue is more critical to the success of PFB than maintaining these facilities through the current phase-down of DOE support. In this regard I am especially concerned about the fate of the Curtiss-Wright facility. In any analysis of the path by which the PFB combined cycle reliability issue will be resolved, the large PFB/gas turbine pilot under construction by Curtiss-Wright is a key step.

My concern is based on the lack of requested funding for this project in Fy 1983 and the very limited release (by DOE) of FY 1982 funds. This has the effect of eroding the funding available for facility construction and forcing more effort into FY 1983. Based on the judgement that the project is critical to the development of PFB, we have offered to work with DOE to provide a practical transition to private sector support. In this regard EPRI is prepared to cooperate in the R&D operation and testing phase of the project. Such participation is, however, contingent on completion of facility construction as originally planned by DOE and Curtiss-Wright. We have also suggested a joint effort with DOE to posture a comprehensive R&D plan that best uses the limited Federal and private resources available to address the key questions facing commercial acceptance of PFB technology. This plan should include both the DOE PFB program and the supporting efforts in the Gas Stream Cleanup program.

An especially damaging effect of the proposed reduction in DOE support for Grimethrope is the effect on the supporting Coal Utilization Research Laboratory (CURL) in England. CURL represents the largest concentration of capability in PFB technology. It largely invented the technology, and has by far the most experience with the day-to-day operation of PFB. If CURL were to disappear, a very real possibility if DOE funding is curtailed, it would be a very serious loss to the overall development of PFB technology It is essential that there exist an experienced competent, flexible group capable of performing pilot scale tests of components and processes in an objective way, generating results that have the confidence of the whole technical community. Those of us who have the job of making comparisons between different approaches and proposals have a particular interest in the existence of a laboratory of this sort. It is perhaps regrettable that the opportunity to develop an equivalent laboratory in the U.S. has not been taken, but it has to be recognized that it would take at least five years and expenditures of at least $16-20 million to try to duplicate this capability. In the meantime the basic development and problem solving efforts necessary to the technical and commercial success of PFB would be stalled.

In hopes of achieving a rapid commercial demonstration in the private sector, EPRI is considering several near-term PFB approaches. These include a "turbocharged" boiler and a repowering system for conventional plants. In the first approach, all power output is developed from steam with the gas turbine providing pressure for the combustor. The second approach is designed to use coal to replace and increase the capacity of exisiting oil-or-gas fired plants while minimizing space and environmental control requirements.

Based on the successful completion of the Grimethorpe and Curtiss-Wright programs, EPRI hopes to implement a commercial PFB prototype with the utility industry. Probable absence of government funding increases the necessity that such an installation provide actual electric generating capacity to interest the necessary private sector funding. Although the EPRI plan contains support for such a project, its full funding mechanism is not yet evident. I emphasize, however, that such a commercial demonstration can only be implemented if the Grimethorpe and Curtiss-Wright results indicate, to users satisfaction, confidence in the reliability and operability of PFB power plants.

CONCLUSIONS

The actions of the Federal government in direct support of energy related research and development and in the form of incentives have a profound effect on the utility industry's ability to develop and use new coal utilization technology. As such, they have major impact on the EPRI program. Federal R&D policy with respect to the utility industry needs to consider the underlying circumstances affecting the industry and the importance of electric energy to the nation's energy, security and economic needs. Continued direct Federal support to share the risk in selected coal combustion and other programs will be necessary if commercialization of improvements is to be achieved. It is doubtful that incentives alone are sufficient to provide the needs of the users of electricity.

Research and development groups are fragile entities that once disbanded cannot be restored. I would, therefore, urge that whatever changes may emerge from the change in Federal energy R&D policy be made gradually so that the system can adjust with minimal damage. Unfortunately, the reduction proposed in the FY1983 DOE budget has occurred so suddenly that

Opportunity for coordination to reduce the shock has been minimal in most programs.

Coal combustion related technology where continued Federal R&D support is most critical must start with PFB. The proposed reduction in funding for the Curtiss-Wright and Grimethorpe/CURL pilot facilities, plus the supporting gas stream cleanup development, may effectively end the promising PFB technology initiative underway in the private sector. This would eliminate the benefit of the major DOE investment to date. In addition, the lack of an assured funding mechanism for the demonstration of both AFB and PFB technology, involving joint Federal/private sector participation, will delay if not cancel their implementation by the utility industry or force later reliance on foreign technology.

Other longer range, higher risk technical programs which should receive higher priority for continued Federal R&D support are materials research, coal preparation and analysis, flue gas cleanup, waste cleanup technology alternative fuel utilization (coal slurries) and university coal research. All of these programs are fundamental to our technological base for efficient and environmentally sound combustion of coal.

There is no shortage of opportunities to improve the efficiency and environmental performance of coal combustion. Our ability to maintain the economy and increase domestic energy self-sufficiency depends on the successful development of these opportunities.

The national interest, therefore, requires a more constructive partnership between government and the private sector in the conduct of coal combustion research and development, and in the implementation of results. This will permit industry to meet our societal needs, as well as compete effectively with industry/government partnerships abroad. EPRI is prepared to do its part in formulating and funding these efforts.

Mr. FUQUA. Thank you very much, Mr. Yeager.
Mr. Spencer.

STATEMENT OF DWAIN F. SPENCER, DIRECTOR, ADVANCED POWER SYSTEMS DIVISION, ELECTRIC POWER RESEARCH INSTITUTE

Mr. SPENCER. Mr. Chairman, it is indeed a pleasure to address this subcommittee on specific elements of the Department of Energy's fiscal year 1983 fossil energy budget and to briefly discuss the status of the Coolwater Coal Gasification Combined Cycle Project. I am Dwain Spencer, director of the Advanced Power Systems Division of the Electric Power Research Institute [EPRI]. In addition, I serve as a member of the Department of Energy's Fossil Energy Advisory Committee. These remarks are my own personal views. They are the result of my close association with the DOE program over the last 5 years, and do not necessarily reflect the positions of EPRI.

As I am sure you are aware EPRI has carried out codevelopment of numerous synthetic fuels projects in conjunction with the Department of Energy. Many of these joint projects are just now demonstrating their value to the country in achieving significant technical objectives and establishing the design bases for pioneer commercial plants or large-scale demonstration facilities. I believe that each of these facilities fulfills an important role in the development of synthetic fuels, and premature termination of DOE funding for the test programs underway would not appear to be a prudent financial decision.

I would like to first address the area of direct liquefaction, and some of the more important synthetic fuels and related coal utilization projects that we have jointly funded with the Department of Energy Fossil Energy Division or in which we closely coordinated our research and development efforts.

One of the best examples of successful cosponsored projects between EPRI and the Department of Energy is our 6,000-ton-per-day pilot plant located at Wilsonville, Alabama. The pilot plant is owned by EPRI, operated by Southern Co. Services, Inc., and Catalytic, Inc., and it is funded two-thirds by DOE and one-third by EPRI. The initial purpose of this facility was to establish the technical design basis for a reduced sulfur, solid fuel which would be capable of being fired in a boiler without scrubbers and yet meet 90 percent sulfur removal requirements. It met these objectives in the 1976–77 time period and led to the development and test of the Kerr-McGee solvent deashing system and selective recycle of particular solvent species in the 1978 through 1980 time period. Subsequently, additional process equipment has been installed in the facility, and it is now an extremely valuable research facility to assess the potential of two-stage liquefaction processes and to produce very low sulfur—0.2 to 0.3 percent—solid products. The total investment in Wilsonville is approximately $25 million by EPRI, $30 million by DOE, and $5 million by Southern Co. Services.

This facility provides a great deal of flexibility to evaluate new process designs and has provided both EPRI and DOE significant insight into the basic liquefaction technology which has aided all of the other direct liquefaction projects. Presently, the annual budget for the facility is $10 to $12 million, with EPRI providing $3.5 to $4 million. We see a continuing value in operating the facility for at least the next 3 to 4 years. Although EPRI may be able to increase its funding support for the facility modestly over the next few years, we will continue to seek DOE and other potential support of $5 to $7 million annually. We, of course, desire that the DOE continue to fund the facility to the maximum extent possible to assure the continuity of effort which has produced so many important technical results to date.

As you know, Wilsonville has provided the design basis for the present Solvent Refined Coal, SRC-1, demonstration plant under design by Air Products/Wheelabrator Frye. Although I am very aware of the present administration's reluctance to commit large amounts of funding for demonstration facilities, I believe that such an expenditure may be warranted for the SRC-1 6,000-ton-per-day demonstration facility. My technical judgment is that at least one direct liquefaction process should be operated at this scale to identify any unforeseen scale-up problems. In the absence of any firm commitments to a pioneer liquefaction facility, it may be prudent for the Government to carry the financial load on this project to assure a sound technical basis when such technologies become economically competitive with petroleum products. Funding could be provided by either the DOE or Synthetic Fuels Corporation as may be deemed most appropriate by the administration and/or Congress.

I would now like to shift my attention to the two large 250-ton-per-day coal liquefaction pilot plants which are presently operating in the United States. EPRI and DOE have cofunded both of these projects, along with numerous other private companies and foreign groups. Both the Exxon donor solvent [EDS] and H-coal process technologies have been shown to be operable on high-sulfur Illinois coal, producing satisfactory yields of synthetic crude oil. Each of the processes is initiating operation on Wyoming subbituminous coal. We know from past experience that process operability and yields may be poorer with this feedstock and the presently planned operating period of 3 to 4 months may not be sufficient to establish a firm design basis on the Wyoming coal.

I am extremely concerned that operations at these two pilot plants will be curtailed prematurely with the termination of DOE funding in fiscal year 1982. My technical judgment is that operation of at least one, and preferably both, of these pilot plants should be extended through at least fiscal year 1983 and perhaps through fiscal year 1984. The annual operating costs for each of these pilot plants is approximately $50 million. As I believe you know, the operating costs of the EDS pilot plant are shared 50/50 with industry, but DOE bears nearly all the operating costs at the H-coal pilot plant. I believe that a firm commitment by DOE of $50 to $60 million to continue operation of these facilities would draw matching private sector funds and allow both pilot plants to continue operating, at least through fiscal year 1983. I seriously recommend your thorough review of this situation and the potential for continued DOE support for these two pilot plants, in which, to date, the country has invested well over $500 million.

In closing: My comments on the direct liquefaction projects in the United States, I would like to bring one additional point to your attention. The purpose of our small and large pilot plant programs is to obtain valid operating data on a wide variety of U.S. coals to develop a firm design basis for demonstration or pioneer commercial plants. Although we have accumulated information for SRC-1 on a wide variety of coals, we have not yet achieved this with either the EDS or H-coal processes at the 250-ton-per-day scale. We need to operate these facilities on four to five coal types to assure ourselves the ability to at least interpolate the performance on other similar coals and hopefully to provide a basis for extrapolation to coals with ash or conversion properties which are beyond our test experience. These are unique facilities, with only one other comparable unit in the world. Let us not prematurely mothball, or even demolish, them before we have thoroughly established their performance potential.

In contrast to my position on direct liquefaction, I believe that DOE should support only a modest effort in further research on the gasification of low rank coals, for example, subbituminous coals or lignite. There are presently four second generation coal gasification technologies being developed entirely with private funding which should provide us a firm basis for the successful gasification of a wide range of U.S. coals. These specific process technologies are Texaco partial oxidation, British Gas/Lurgi slagging gasifier, Shell partial oxidation, and the Allis-Chalmers Kilngas process.

In my judgment, these four technologies, if successfully demonstrated, will provide fuel gas for retrofit of boilers and combustion turbines integrated gasification combined plants, and chemical synthesis plants to produce ammonia, methanol, and petrochemicals from coal. In each case, either a large pilot plant is operating or in construction and plans are being made for the first full-scale demonstration of at least three of these technologies within the United States or in Western Europe. As I will discuss in a few moments, EPRI has played a crucial role in completing the funding for the 100-megawatt Coolwater integrated coal gasification combined cycle project using the Texaco process.

Although we are aware of numerous other coal gasification technologies under development, we doubt that any will show significantly enhanced technical or economic performance relative to those discussed above, at least for power generation. There may be advantages to other technologies for synthetic natural gas production; however, I will not discuss that potential need. In summary, I think that the economic marketplace will lead to the development of sufficient coal gasification technologies; thus the Government should limit its activities to perhaps price supports and/or loan guarantees through the Synthetic Fuels Corporation.

My final comments relative to the Department of Energy fossil energy programs relate to two advanced power generation technologies, namely the high temperature turbine technology and open cycle MHD programs. Although the Department of Energy is planning to curtail or terminate these activities, it appears to be a paradox. DOE states that they are focusing on high-risk, long-term technology research. In my opinion, both of these technologies clearly fall in that category.

I believe that continued research and development on advanced blade and vane cooling techniques for combustion turbines is valuable and may not be continued in the private sector if DOE terminates their support. Advanced air and water cooling technology should be supported by DOE with an initial target of increasing turbine blade inlet temperatures from 2,000° to 2,300–2,400° Fahrenheit and in the longer term to 2,600 to 3,000° Fahrenheit. If such turbines could be developed and operated reliably, coal gasification combined cycle plants would increase in efficiency from 35 to 36 percent to 40 to 42 percent and such research would have provided a major improvement in plant performance. Although DOE may not wish to focus on a single technology for demonstration, a technology support program of $20 to $25 million annually appears warranted and viable.

Although, in my opinion, there are greater technical problems with open cycle MHD, even greater overall efficiencies are promised if the technology can be successfully developed. Although I presently do not see a large economic "carrot" for this technology, high efficiency systems for utilizing coal would seem to be in the national interest. Therefore, I strongly recommend that the Department of Energy restore the support for the Component Development and Integration Facility, CDIF, in Montana, and perhaps other related test facilities. Further, I recommend that modifications be made to the CDIF which will allow the test of critical seed recovery approaches in the presence of ash. I envision that a continuing program of $35 to $40 million annually for the next 4 years would provide us a firm basis to judge the value of open cycle MHD technology on a variety of coals.

In summary, I have recommended additions to the Department of Energy fossil energy budget of approximately $110 to $140 million in fiscal year 1983 for those areas in which I have the most direct program knowledge. These additions would provide a well-balanced national fossil energy program and permit meaningful completion of significant projects which have been previously initiated or have longer term research and development objectives. During 1983, EPRI will be expending nearly $50 million on related technical development of synthetic fuels for electric power production. I certainly believe a $250 to $300 million commitment by the Federal Government is prudent and warranted.

And finally, Mr. Chairman, I would like to briefly discuss the Coolwater Gasification Combined Cycle Project.

In my opinion, one of the truly significant events in the development of synthetic fuels processes in the country occurred last December when the EPRI board of directors committed up to $126 million to close the funding for the coolwater 100-megawatt integrated gasification combined cycle plant. This project is a partnership arrangement with entirely private sector funding to demonstrate the Texaco partial oxidation technology in an integrated gasification combined cycle powerplant. This commitment has been made since we judge the coal gasification process technology is close to economic viability. The present partnership is composed of Southern California Edison Co., Texaco, the General Electric Co., Bechtel Inc., the Japanese Cool Water Program Partnership, led by Tokyo Electric Power Co., and ESEERCO, in addition to EPRI. Although our board of directors assured funding closure, we are continuing discussions with domestic and foreign companies to share in the project benefits and costs through project participation. In addition, we are seeking a price guarantee/loan guarantee for a small portion of the project from the Synthetic Fuels Corporation; however, project continuation is not dependent on an affirmative response from the Synthetic Fuels Corporation.

The coolwater project is a $300 million effort to design, construct, test, and operate the Nation's first integrated gasification combined cycle plant at a utility site. The demonstration facility will employ a 1,000-ton-per-day oxygen blown Texaco gasifier to produce approximately 100 megawatts of net power from a slightly modified commercial General Elecric combined cycle unit. Bechtel is the engineering and construction contractor for the unit which is being constructed at Southern California Edison's Coolwater generating station near Daggett, Calif.

Engineering design of the plant is over 50 percent complete, all environmental permits are in hand, all major equipment has been ordered, and civil construction work has been underway since mid-December of last year. The plant is being designed to meet stringent California air quality requirements on a high-sulfur Illinois bituminous coal. Specifically, the plant is being designed for 97 percent sulfur removal using Selexol acid gas removal process—the emissions will be less than 0.2 pounds per million Btu—and nitrogen oxide and particulates emission design requirements are 0.14 and 0.01 pounds per million Btu, respectively.

The plant should begin its predemonstration test period in June 1984, and initiate a 6½-year operating period in November 1984. The plant is being designed so that the fuel gas produced can also be fired in one of the existing boilers at the Coolwater site, as well as used in the integrated gasification combined cycle plant. We believe that this project has the potential to lead to a whole new way of utilizing coal in an economically acceptable, environmentally clean manner. Our task now is to clearly demonstrate that this potential can be realized. We, at EPRI, along with the power industry and potential suppliers, are committed to do so and believe that we will be successful in this undertaking.

Mr. Chairman, thank you for the opportunity to address your committee. If I or my staff can be of further service to you, please call upon us. We, indeed, see the decade of the 1980's establishing the synthetic fuels technology basis to provide the commercial plant options from domestic sources in the 1990's and beyond.

Thank you very much.

[The prepared statement of Mr. Spencer follows:]

REMARKS ON THE DEPARTMENT OF ENERGY FY'83 BUDGET AND A SUMMARY OF THE COOL WATER COAL GASIFICATION PROJECT

Dwain F. Spencer
Director
Advanced Power Systems Division

Hearing on FY1983 Authorization for the
Department of Energy Fossil Energy Research
and Development Program

Presented to:

Subcommittee on Energy Development and Application

Committee on Science and Technology
United States House of Representatives
Washington, D.C.

23 March 1982

I) Introduction

Mr. Chairman, it is indeed a pleasure to address this subcommittee on specific elements of the Department of Energy's FY'83 Fossil Energy budget and to briefly discuss the status of the Cool Water coal gasification combined cycle project. I am Dwain Spencer, Director of the Advanced Power Systems Division of the Electric Power Research Institute (EPRI). In addition, I serve as a member of the Department of Energy's Fossil Energy Advisory Committee. These remarks are my own personal views. They are the result of my close association with the DOE program over the last five years and do not necessarily reflect the positions of EPRI.

As I am sure you are aware, EPRI has carried out co-development of numerous synthetic fuels projects in conjunction with the Department of Energy. Many of these joint projects are just now demonstrating their value to the country in achieving significant technical objectives and establishing the design bases for Pioneer commercial plants or large scale demonstration facilities. I believe that each of these facilities fulfills an important role in the development of synthetic fuels, and premature termination of DOE funding for the test programs underway, would not appear to be a prudent financial decision.

II) Specific Remarks on the DOE FY'83 Fossil Energy Program

A. Direct Liquefaction

I would now like to discuss some of the more important synthetic fuels and related coal utilization projects that we have jointly funded with DOE-FE or in which we closely coordinated our research and development efforts. One of the best examples of a successful co-sponsored project between EPRI and DOE-FE is our six ton/day pilot plant located at Wilsonville, Alabama. The pilot plant is owned by EPRI, operated by Southern Company Services, Inc., and Catalytic, Inc., and is funded 2/3 by the DOE and 1/3 by EPRI. The initial purpose of this facility was to establish the technical design basis for a reduced sulphur, solid fuel which would be capable of being fired in a boiler without scrubbers and yet meet 90 percent sulphur removal requirements. It met these objectives in the 1976-77 time period and led to the development and test of the Kerr-McGee solvent deashing system and selective recycle of particular solvent species in the 1978-80 time period. Subsequently, additional process equipment has been installed in the facility, and it now is an extremely valuable research facility to assess the potential of two stage liquefaction processes and to produce very low sulphur (0.2-0.3 percent) solid products. The total investment in Wilsonville is approximately $25 million by EPRI, $30 million by DOE, and $5 million by Southern Company Services.

This facility provides a great deal of flexibility to evaluate new process designs and has provided both EPRI and DOE significant insight into the basic liquefaction technology which has aided all of the other direct liquefaction projects. Presently, the annual budget for the facility is $10-12 million, with EPRI providing $3.5 to 4.0 million. We see a continuing value in operating the facility for at least the next 3-4 years. Although EPRI may be able to increase its funding support for the facility modestly over the next few years, we will continue to seek DOE and other potential support of $5-7 million annually. We, of course, desire that the DOE continue to fund the facility to the maximum extent possible to assure the continuity of effort which has produced so many important technical results to date.

As you know, Wilsonville has provided the design basis for the present Solvent Refined Coal (SRC-I) demonstration plant under design by Air Products/Wheelebrator Frye. Although I am very aware of the present Administration's reluctance to commit large amounts of funding for demonstration facilities, I believe that such an expenditure may be warranted for the SRC-I 6000 ton/day demonstration facility. My technical judgment is that at least one direct liquefaction process should be operated at this scale to identify any unforeseen scale-up problems. In the absence of any firm commitments to a Pioneer liquefaction facility, it may be prudent for the government to carry the financial load on this project to assure a sound technical basis when such technologies become economically competitive with petroleum products. Funding could be provided by either the DOE or Synthetic Fuels Corporation (SFC) as may be deemed most appropriate by the Administration and/or Congress.

I would now like to shift my attention to the two large (250 ton/day) coal liquefaction pilot plants which are presently operating in the U.S. EPRI and DOE have cofunded both of these projects, along with numerous other private companies and foreign groups. Both the Exxon Donor Solvent (EDS) and H-Coal process technologies have been shown to be operable on high sulphur

Illinois coal, producing satisfactory yields of synthetic crude oil. Each of the processes is initiating operation on Wyoming subbituminous coal. We know from past experience that process operability and yields may be poorer with this feedstock and the presently planned operating periods of 3-4 months may not be sufficient to establish a firm design basis on the Wyoming coal.

I am extremely concerned that operations at these two pilot plants will be curtailed prematurely with the termination of DOE funding in FY'82. My technical judgment is that operation of at least one, and preferably both, of these pilot plants should be extended through at least FY'83 and perhaps through FY'84. The annual operating costs for each of these pilot plants is approximately $50 million. As I believe you know, the operating costs of the EDS pilot plant are shared 50-50 with industry, but DOE bears nearly all the operating costs at the H-Coal pilot plant. I believe that a firm commitment by DOE of $50-$60 million to continue operation of these facilities would draw matching private sector funds and allow both pilot plants to continue operating, at least through FY'83. I seriously recommend your thorough review of this situation and the potential for continued DOE support for these two pilot plants, in which, to date, the country has invested well over $500 million.

In closing my comments on the direct liquefaction projects in the U.S., I would like to bring one additional point to your attention. The purpose of our small and large pilot plant programs is to obtain valid operating data on a wide variety of U.S. coals to develop a firm design basis for demonstration or Pioneer commercial plants. Although we have accumulated information for SRC-I on a wide variety of coals, we have not yet achieved this with either the EDS or H-Coal processes at the 250 ton/day scale. We need to operate these facilities on four to five coal types to assure ourselves the ability to, at least, interpolate the performance on other similar coals and hopefully to provide a basis for extrapolation to coals with ash or conversion properties which are beyond our test experience. These are unique facilities, with only one other comparable unit in the world. Let us not prematurely mothball, or even demolish, them before we have thoroughly established their performance potential.

B. Coal Gasification for Electric Power Production

In contrast to my position on direct liquefaction, I believe that DOE should support only a modest effort in further research on the gasification of low rank coals, i.e., subbituminous coals or lignite. There are presently four second generation coal gasification technologies being developed entirely with private funding which should provide us a firm basis for the successful gasification of a wide range of U.S. coals. These specific process technologies are Texaco Partial Oxidation, British Gas/Lurgi Slagging Gasifier, Shell Partial Oxidation, and the Allis-Chalmers Kilngas process.

In my judgment, these four technologies, if successfully demonstrated will provide fuel gas for retrofit of boilers and combustion turbines integrated gasification combined plants, and chemical synthesis plants to produce ammonia, methanol, and petrochemicals from coal. In each case, either a large pilot plant is operating or in construction and plans are being made for the first full scale demonstration of at least three of these technologies within the U.S. or Europe. As I will discuss in a few moments, EPRI has played a crucial role in completing the funding for the 100 MWe Cool Water integrated coal gasification combined cycle project using the Texaco process.

Although we are aware of numerous other coal gasification technologies under development, we doubt that any will show significantly enhanced technical or economic performance relative to those discussed above, at least for power generation. There may be advantages to other technologies for synthetic natural gas production; however, I will not discuss that potential need. In summary, I think that the economic marketplace will lead to the development of sufficient coal gasification technologies; thus the government should limit its activities to perhaps price supports and/or loan guarantees through the SFC.

C. Power Generation

My final comments relative to the DOE-FE programs relate to two advanced power generation technologies, namely the High Temperature Turbine Technology and Open Cycle Magnetohydrodynamic (OCMHD) programs. Although DOE is planning to curtail or terminate these activities, it appears to be a paradox. DOE states that they are focusing on high risk, long term technology research. In my opinion, both of these technologies clearly fall in that category.

I believe that continued research and development on advanced blade and vane cooling techniques for combustion turbines is valuable and may not be continued in the private sector if DOE terminates their support. Advanced air and water cooling technology should be supported by DOE with an initial target of increasing turbine inlet temperatures from 2000°F to 2300-2400°F and in the longer term to 2600-3000°F. If such turbines could be developed and operated reliably, coal gasification combined cycle plants would increase in efficiency from 35-36 percent to 40-42 percent and such research would have provided a major improvement in plant performance. Although DOE may not wish to focus on a single technology for demonstration, a technology support program of $20-25 million annually appears warranted and viable.

Although, in my opinion, there are greater technical problems with OCMHD, even greater overall efficiencies are promised if the technology can be successfully developed. Although I presently do not see a large economic "carrot" for this technology, high efficiency systems for utilizing coal would seem to be in the national interest. Therefore, I strongly recommend that DOE restore the support for the Component Development and Integration Facility (CDIF) in Montana, and perhaps other related test facilities. Further, I recommend that modifications be made to the CDIF which will allow the test of critical seed recovery approaches in the presence of ash. I envision that a continuing program of $35-50 million annually for the next four years would provide us a firm basis to judge the value of OCMHD technology on a variety of coals.

D. Summary of Remarks on DOE Budget

In summary, I have recommended additions to the DOE-FE budget of approximately $110-140 million in FY'83 for those areas in which I have the most direct program knowledge. These additions would provide a well balanced national fossil energy program and permit meaningful completion of significant projects which have been previously initiated or have longer term research and development objectives. During 1983, EPRI will be expending nearly $50 million on related technical development of synthetic fuels for electric power production. I certainly believe a $250-300 million commitment by the federal government is prudent and warranted.

III. The Cool Water Coal Gasification Combined Cycle Project

In my opinion, one of the truly significant events in the development of synthetic fuels processes in the country occurred last December when the EPRI Board of Directors committed up to $126 million to close the funding for the Cool Water 100 MWe Integrated Gasification Combined Cycle Project. This project is a partnership arrangement with entirely private sector funding to demonstrate the Texaco partial oxidation technology in an integrated gasification combined cycle (IGCC) power plant. This commitment has been made since we judge the coal gasification process technology is close to economic viability. The present partnership is composed of Southern California Edison Co., Texaco, Inc., the General Electric Company, Bechtel, Inc., the Japanese Cool Water Program Partnership, led by Tokyo Electric Power Co., and ESEERCO, in addition to EPRI. Although our Board of Directors assured funding closure, we are continuing discussions with domestic and foreign companies to share in the project benefits and costs through project participation. In addition, we are seeking a price guarantee/loan guarantee for a small portion of the project from the SFC; however, project continuation is not dependent on an affirmative response from the SFC.

The Cool Water Project is a $300 million effort to design, construct, test, and operate the nation's first IGCC plant at a utility site. The demonstration facility will employ a 1000 ton/day oxygen blown Texaco gasifier to produce approximately 100 MW of net power from a slightly modified commercial General Electric combined cycle unit. Bechtel, Inc., is the engineering and construction contractor for the unit which is being constructed at Southern California Edison's Cool Water generating station near Daggett, California.

Engineering design of the plant is over 50 percent complete, all environmental permits are in hand, all major equipment has been ordered, and civil construction work has been underway since mid December of last year. The plant is being designed to meet stringent California air quality requirements on a high sulphur Illinois bituminous coal. Specifically, the plant is being designed for 97 percent sulphur removal using Selexol acid gas removal process (emissions will be less than 0.2 pounds per million Btu) and nitrogen oxide and particulates emission design requirements are 0.14 and 0.01 pounds per million Btu, respectively.

The plant should begin its pre-demonstration test period in June 1984 and initiate a 6-1/2 year operating period in November 1984. The plant is being designed so that the fuel gas produced can also be fired in one of the existing boilers at the Cool Water site, as well as used in the IGCC plant. We believe that this project has the potential to lead to a whole new way of utilizing coal in an economically acceptable, environmentally clean manner. Our task now is to clearly demonstrate that this potential can be realized. We, at EPRI, along with the power industry and potential suppliers, are committed to do so and believe that we will be successful in this undertaking.

Mr. Chairman, thank you for the opportunity to address your Committee. If I or my staff can be of further service to you, please call upon us. We, indeed, see the decade of the 80s establishing the synthetic fuels technology basis to provide the commercial plant options from domestic sources in the 1990s and beyond.

Mr. **Fuqua**. Thank you, Mr. Spencer.

And now to present our next witness, I would like to call on the distinguished Member from Tennesee, Mr. Gore.

Mr. **Gore**. Thank you very much, Mr. Chairman.

It is my pleasure to introduce to this committee for her first appearance here, Dr. Susan Wu, who lives in my district and is a personal friend, but who has also been a leader in the field of MHD for some 15 or 16 years. She is a professor of engineering as well as the program manager at the University of Tennessee Space Institute. She is well known as a theorist and is responsible for a long list of technical discoveries in this field. She has led the R. & D. portion of the MHD effort at UTSI since its inception and she is recognized not just in this country but in other countries as well. She participated in the United States-Soviet joint program. She was program chairman of the AIAA's 14th World Symposium on MHD and chairman of the Plasma Dynamics Technical Committee. She has been selected a co-editor of the upcoming special issue of the Journal of Energy on MHD which is to be published this summer, and we are very proud of her in Tennessee, and I commend her testimony to the committee.

Mr. **Fuqua**. Thank you, Mr. Gore. And after all that we are anxious to hear you, Dr. Wu, and you are certainly welcome here today.

STATEMENT OF Y. C. L. (SUSAN) WU, PROFESSOR AND ADMINISTRATOR, ENERGY CONVERSION RESEARCH AND DEVELOPMENT PROGRAMS, THE UNIVERSITY OF TENNESSEE SPACE INSTITUTE

Dr. **Wu**. Thank you, Congressman Gore, Mr. Chairman, and members of the subcommittee.

I appreciate this opportunity to testify regarding the MHD program of the Department of Energy. DOE and its predecessors have invested $418 million in this program nationwide over the past 11 years. Significant advances have been made in all areas, especially in pollution control. After putting the coal-fired flow facility into operation last year, we were able to reconfirm data taken earlier in smaller scale tests, showing that the pollution control process developed at the University of Tennessee Space Institute is capable of reducing sulfur dioxide emissions from burning high-sulfur coal to levels much below EPA standards. At the same time we were also able to reduce the nitrogen oxide content of the exhaust to significantly lower levels. It should also be noted that this pollution control work is not necessarily dependent on the plant being an MHD plant, but can be applied to more conventional powerplants as well.

Despite the importance to our Nation of this progress in reducing air pollution, the project is threatened with extinction if the Congress does not provide at least $31 million to the overall MHD program for fiscal year 1983, and at the same time prevent DOE from terminating the program this year. Premature termination not only will cause the Nation to lose its investment in the MHD technology and its spinoffs, but also would prevent the orderly archiving of the information thus far gained for future use.

The MHD generator, for example, has the unique features of rapid startup and light weight. These features have important military implications. The Soviet Union developed a mobile MHD unit called PAMIR mounted on two trucks. It was claimed that the unit was used for geological research. Certainly such a unit can be used for other military use as well.

MHD is a conversion scheme that converts heat to electricity directly. It can be used with heat sources other than chemical energy released by combustion. Such possibilities include nuclear reactors, both fission and fusion, using the MHD process to generate electricity.

Spinoff technology from MHD is not a negligible consideration. We have active requests to use the CFFF facility for large projects in non-MHD technology to be funded by industry. In addition, MHD as a technology has many applications and similarities with other high-temperature technologies. Therefore, significant justification exists to pursue MHD work on its own scientific merit.

To substantiate my arguments, I would like to briefly go over some of the technical achievements and issues and also the funding situation. My prepared statement had contained a very substantial amount of technical information. In order to shorten the time maybe we could just go over some of the highlights.

First of all, if you would please turn to page 4 of my prepared statement. Page 4 has a diagram that shows the funding distribution for the $418 million, starting from fiscal 1971 to fiscal 1982.

During this period $418 million was spent, and it initially started at the Office of Coal Research and later evolved into DOE. Roughly one-third of the money was spent on component development, which included combustor, channel, superconducting magnet, and so on. Another one-third was spent in facilities. One is the Component Development Integration Facility, CDIF, in Montana, and the other is the Coal Fired Flow Facility, CFFF, in Tennessee. Both of the facilities are completed within budget and schedule, and are now ready to produce critically needed data.

At the beginning of 1971, when the Office of Coal Research started looking into MHD for possible use for central power, there were very few groups working on MHD. Most of the people working on MHD were supported by the Air Force and Navy. At that time the generator had not operated more than maybe a few seconds. There was no experimental work at all with coal, there was no coal combustor, there was no superconducting magnet, there was no experimental data on coal. There were only very limited and moderate efforts on system analysis. Some of the critics of MHD viewed these as insurmountable barriers, although the MHD community has never believed that. After 11 years, great achievements have been made in all these areas.

In order to shorten my presentation let me just look into two important areas: One is the environmental control area and another is the generator area.

If you look at the diagram on page 5 it shows the SO_2 removal. This shows experimental data obtained at the University of Tennessee Space Institute. A 4-percent sulfur coal such as Illinois No. 6 coal we see that with the addition of potassium—which incidentally is used not to eliminate sulfur dioxide but is used for providing enough electrical conductivity to the plasma—we would be able to eliminate sulfur emission to a very, very low level. With 1 percent of potassium by weight in a total flow where it is required for the electricalconductivity will allow us to meet EPA standards for 4 percent sulfur coal.

The next page shows the data obtained, also at the University of Tennessee Space Institute, on the nitrogen oxide control. Staged combustion has been used to control NO_x for many technologies. This also is true for MHD. As we can see, initially the coal is combusted without enough oxygen, we can reduce the NO_x to essentially nothing at about 15 percent of oxygen deficiency. After further burning at a lower temperature the result is still much below the EPA standard.

The next page, figure 5, shows the MHD generator performance. For the MHD generator there are two major issues, one is generator performance; another is electrode lifetime.

This shows some black circles and triangles and some white ones. The black ones indicate those where we have already obtained experimentals results. The others are design values or calculated values. The results with coal are of small size and low-magnetic field. We must continue our experimental effort with coal at larger facilities, which means both at CFFF and CDIF.

The channel lifetime—the most important problem with the channel lifetime is the electrode life. Recently AVCO Everett Research Laboratory completed a 1,000-hour power test in a simulated coal environment operating under central power electric stress conditions. Extrapolation of the lifetime from this 1,000-hour tests range from 5,000 to 8,000 hours is expected.

We feel that with these achievements we are at a stage to proceed foward with the remaining problems that need to be solved.

Let me go to the conclusion.

The MHD program covers the entire system rather than one component and all areas have made long strides since the beginning of the MHD program in DOE. We are now facing a critical point of decision, to either continue to pursue this promising technology or abandon it without obtaining the results from our investment. The current uncertainty in the MHD program has already taken a heavy toll. Large numbers of well-trained professionals have left this field since last spring.

Our national strength comes from our advanced technology which results directly from R. & D. The Federal Government must assume the responsibility of basic R. & D. in areas concerning national economic well being and security. I realize that the point of commercialization of any technology depends on more than the maturity of the technology alone. Risk, market, and general economic conditions all play important roles, and, therefore, only private industries, through their own evaluation, can determine at what point they can commit to commercialization.

My opinion is that the Federal Government has been very effective in advancing technology, especially in the areas involving long-range results and high risks. However, effective programs cannot be run without a steady Federal commitment. Large funding fluctuations, including the continued threat of early termination, prevent effective program planning and execution.

Let me conclude my testimony by quoting an article which appeared in the March 1982 issue of Energy Insider. The article cited Deputy Secretary Davis:

> Basic research is widely recognized to be needed for the Nation's future economic development. But basic research does not command a high priority in private firms, particularly during times of high inflation and high interest rates. Industries such as electric and gas utilities that are subject to strict regulations are not permitted the rates of return that would allow them to invest in developing advanced electric generation technologies such as magnetic fusion or breeder reactors.

The MHD program is such a developing technology, and the potential payoffs are worth seeing it through to completion.

Thank you very much for your attention.

[The prepared statement of Dr. Wu follows:]

TESTIMONY
ON THE MAGNETOHYDRODYNAMIC (MHD) PROGRAM OF THE DEPARTMENT OF ENERGY (DOE)

PREPARED FOR THE
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
COMMITTEE ON SCIENCE AND TECHNOLOGY

MARCH 23, 1982

BY
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Mr. Chairman and Members of the Subcommittee, I appreciate this opportunity to testify regarding the MHD Program of the Department of Energy. DOE and its predecessors have invested $418 million in this program nationwide over the past eleven years. Significant advances have been made in all areas, especially in pollution control. After putting the Coal Fired Flow Facility (CFFF) into operation last year, we were able to reconfirm data, taken earlier in smaller scale tests, showing that the pollution control process developed at The University of Tennessee Space Institute (UTSI) is capable of reducing sulfur dioxide emissions from burning high sulfur coal to levels much below EPA standards. At the same time we were also able to reduce the nitrogen oxide content of the exhaust to significantly lower levels. It should also be noted that this pollution control work is not necessarily dependent on the plant being an MHD plant, but can be applied to more conventional power plants as well.

Despite the importance to our Nation of this progress in reducing air pollution, the project is threatened with extinction if the Congress does not provide at least $31 million to the overall MHD program for 'FY83, and at the same time prevent DOE from terminating the program this year. Premature termination not only will cause the Nation to lose its investment in the MHD technology and its spinoffs, but also would prevent the orderly archiving of the information thus far gained for future use.

The MHD generator for example has the unique features of rapid start-up and light weight. These features have important military implications. The Soviet Union developed a mobile MHD unit (PAMIR) mounted on two trucks. This unit is claimed to be used for geological research.

MHD is a conversion scheme that converts heat to electricity directly. It can be used with heat sources other than chemical energy released by combustion. Such possibilities include nuclear reactors, both fission and fusion, using the MHD process to generate electricity.

Spinoff technology from MHD is not a negligible consideration. We have active requests to use the CFFF facility for large projects in non-MHD technology to be funded by industry. In addition, MHD as a technology has many applications and similarities with other high temperature technologies. Therefore, significant justification exists to pursue MHD work on its own scientific merit.

To substantiate my arguments, I would like to present briefly the work status and technical issues, and then state my broader thinking on the federal role in R&D, particularly in energy R&D.

I. INTRODUCTION

This committee is very familiar with the MHD power generation system already. Nevertheless let me briefly review the process as shown in Figure 1.

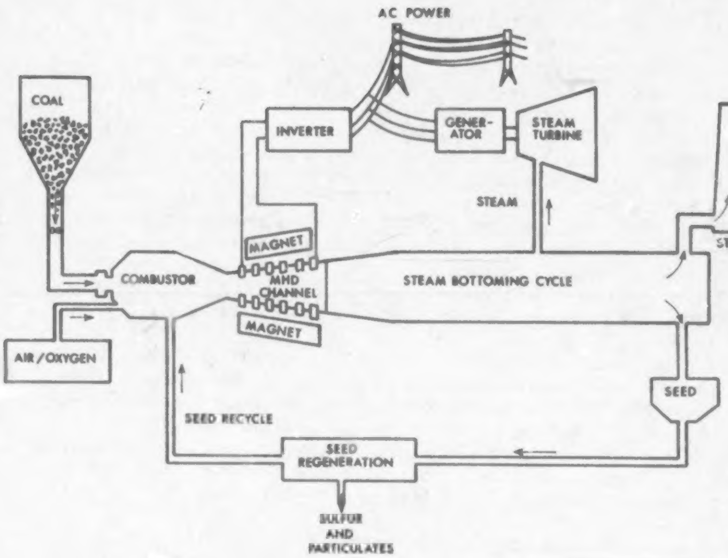


Figure 1. MHD Combined Cycle Power Plant

The MHD power plant is a combined cycle plant. The upstream topping plant is the MHD cycle, while the downstream is the steam bottoming cycle. The MHD power generation is based on the same principles as the conventional generator. The difference is that the solid conductors (wires) are replaced by a gas that conducts electricity.

The combined efficiency can be significantly higher than the steam plant alone. Systems analysis carried out by GE, Westinghouse, AVCO and Gilbert Associates have shown that a 30% increase over the conventional steam plant is expected in the early MHD/steam plants, while 50% increase is possible with mature plants.

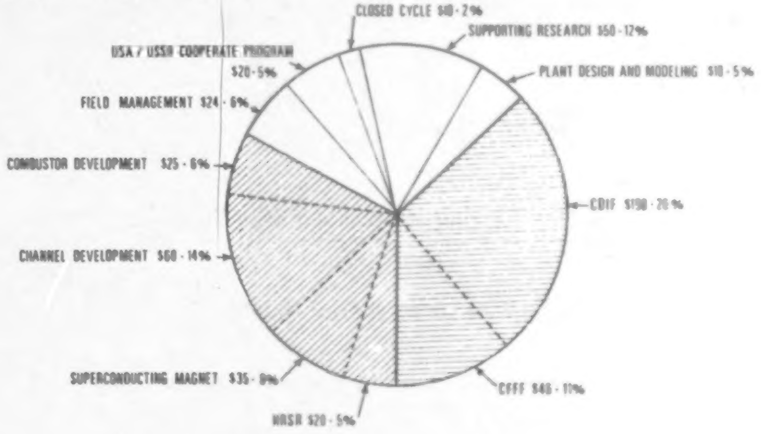
The MHD-coal process itself is less polluting, as compared to conventional coal fired plants. For example, let us consider air pollution caused by sulfur dioxide emissions. The combustion gas produced by burning coal must be "seeded" with an easily ionizable material, and the one most commonly used is potassium carbonate (K_2CO_3). Potassium provides the needed electrical conductivity in the MHD process, while serving as a scrubbing agent in the downstream steam plant. If sulfur is present in the flue gas, it will combine with potassium to form a solid, potassium sulfate (K_2SO_4), which can be removed by particulate collection equipment. This will eliminate the expensive scrubbing required to reduce sulfur emission.

Another polluting agent we must consider carefully is nitrogen oxides. The flame temperature and electrical conductivity of the combustion gas plasma have maximum values at fuel rich conditions. To take advantage of this fact, staged combustion is used. It has been shown that staged combustion is an effective method to reduce NO_x emission. Experimental results obtained by various organizations have verified this fact for an MHD system.

From a passive point of view, the above mentioned higher efficiency of the combined cycle plant itself will lead automatically to lower pollution for each kilowatt hour produced.

II. FUNDING SUMMARY

The MHD program for central power generation started in 1971 at the Office of Coal Research and later evolved to DOE. During the period of FY'71 to FY'82, $418 million has been spent on this program. The following diagram shows the functional distribution of funding. Roughly one third of the money was spent on component development, one third on experimental facilities, while the remainder was spent on various other activities including diagnostics, support R&D, system analysis, modeling and others. In 1971 there were only a few research groups and laboratories working on MHD, mainly supported by the Air Force and Navy. There was no such experimental work using coal. Today two experimental facilities using coal have been built, the Component Development Integration Facility (CDIF) in Montana and the Coal Fired Flow Facility (CFFF) in Tennessee, and both are ready to produce critically needed data. In addition, a large clean fuel facility has been in operation at the Air Force Arnold Engineering Development Center (AEDC) in Tennessee.



MHD FUNDING DISTRIBUTION FY 71-FY82

total $418 million

▨ FACILITY, 154 MILLION (37%)

▤ COMPONENT DEVELOPMENT, 140 MILLION (33%)

○ OTHER ACTIVITIES, 124 MILLION (30%)

Figure 2. MHD Program Funding Distribution

b. NO_x Control

Staged combustion is used for both increasing the electrical conductivity as well as NO_x control. Optimum electrical conductivity in an MHD generator occurs around a condition that is 15% deficient in oxygen. Figure 4 shows the NO_x concentration versus oxygen availability obtained at UTSI. The NO_x level is nearly zero prior to secondary combustion at 85% stoichiometry. After secondary combustion, the final NO_x level is about 0.2 lb/10^6 BTU. This is far below the EPA limit.

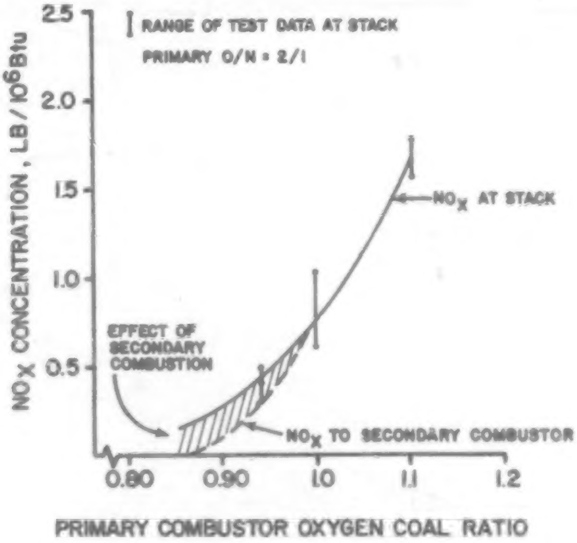


Figure 4. Effect of Primary Combustion Stoichiometry and Secondary Combustion

c. Particulate Control

There are two principal areas of concern about particulate control in MHD power systems: 1) the nature of the particulates and the optimum method for their control; and 2) the fate of trace elements, especially those that tend to be more volatile and might be enriched in the fly ash not collected in the system and thus dispersed to the atmosphere. Actually, these concerns exist for conventional power plant practice, particularly since there is uncertainty as to what control technology will best meet the New Source Performance Standards (NSPS) of 0.03 lb particulates/10^6 BTU. The higher temperatures and addition of potassium compounds introduce questions that must be answered for MHD systems. Preliminary results obtained at UTSI did not show any unusual phenomena. However additional data are required. In order to obtain these data, it is necessary to install particulate collection equipment which we are proposing to do next year.

2. MHD Generator

a. Generator Performance

We can measure generator performance in terms of thermal input versus power output (enthalpy extraction). Figure 5 shows the enthalpy extractions as a function of the size of the generator. As the generator size increases so does the enthalpy extraction. The AEDC experiments using clean fuel, and the CDIF using oil with ash injection, have shown good results already and they are moving forward to achieve the designed values. However all of the experimental results obtained today using coal, and oil with ash injection, are of small size and low magnetic field. It is necessary to obtain experimental results at larger sizes and higher magnetic field strengths in order to gain scale-up knowledge.

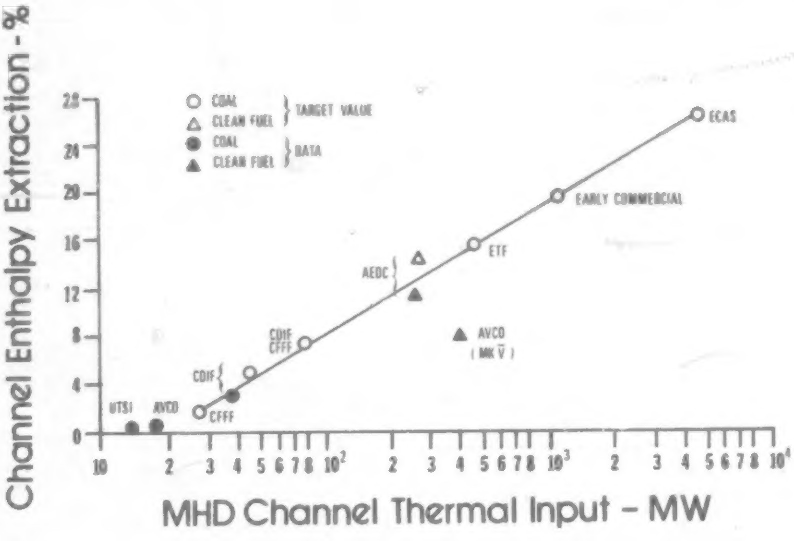


Figure 5. Enthalpy Extraction as Function of Thermal Input

b. Channel Life Time

Recently AVCO Everett Research Laboratory completed a 1000 hour power test in a simulated coal environment, operating under central power electrical stress conditions. The materials used were Platinum and platinum/stainless steel clad copper anodes. Extrapolation of the lifetime of present state-of-art MHD anodes based on the 1000 hour test is in the range of 5000 to 8000 hours. The amount of platinum required for commercial baseload MHD channels presents neither a serious cost penalty nor a problem of availability.

3. Superconducting Magnet

A 5 Tesla superconducting magnet (30 ton) designed and built by Argonne National Laboratory was successfully operated over a two year period in the U.S. - U.S.S.R. joint facility (U-25B) at the High Temperature Institute in Moscow. A much larger 6T 200-ton magnet designed and built by the same team for CFFF was successfully tested last fall. However due to the lack of funds, the magnet is stored at ANL. This is the largest dipole superconducting magnet ever built. General Electric Co. is under contract with DOE to build a large superconducting magnet for CDIF. Due to budget constraint, that project is now in limbo.

4. Coal Combustor

There is a great deal of similarity between rocket engines and MHD combustors. Aerospace technology is being directly adapted to develop MHD combustors. As a result, over a relatively short time compared with the history of coal combustion, important strides have been made for both full slag carry-over combustors developed at UTSI and slag rejection combustors developed at TRW. The MHD combustors developed for approximately 20 MW_t have been successfully operated under high temperature and pressure with very low heat loss. These advanced coal combustors can be used in other applications, coal gasifiers for example.

The most critical parameter in rocket engine development is scale-up, which we also anticipate to be true for the MHD combustors. With the vast amount of scale-up knowledge existing in rocket engines, we can shorten the development time required of MHD combustors significantly. However there are important differences between coal and rocket fuels, both in their forms and consistencies. These effects must be investigated as relating to methods of coal injection, slag separation and others.

5. Heat Recovery/Seed Recovery (HRSR) System

The CFFF low mass flow test train is a complete system which includes both the upstream MHD cycle as well as the downstream bottoming cycle. The CFFF HRSR design is based on the extensive industrial experience of Kraft boilers. The downstream test components currently consists of a radiant furnace, secondary combustor and a material test module.

Experiments for NO_x and SO_2 control are scheduled this year. Plans are made for designing the remaining portion of the test train, namely the superheater, air heater and particulate collection equipment. These equipment items are necessary to obtain needed data, but they are not in existence today.

6. Diagnostics and Other Supporting Research

Due to the harsh environment of the coal fired MHD system, conventional measurement techniques are not applicable in many cases. As a result, new or improved diagnostic tools must be developed. Both Mississippi State University and Stanford University have made great advancements in this area. Westinghouse is developing an inverter system for CDIF. Materials, slag properties, computational techniques, systems analysis and other supporting R&D areas carried out by various contractors, including MIT, ANL, STD, UTSI, Stanford, NASA-Lewis, AVCO, GE, Westinghouse, NBS, Battelle- Northwest and others, have all made great contributions toward the eventual commercialization of MHD power generation.

IV. CONCLUSION

The MHD program covers the entire system rather than one component and all areas have made long strides since the beginning of the MHD program in DOE. We are now facing a critical point of decision, to either continue to pursue this promising technology or abandon it without obtaining the results from our investment. The current uncertainty in the MHD program has already taken a heavy toll. Large numbers of well trained professionals have left this field since last spring.

Our national strength comes from our advanced technology which results directly from R&D. The federal government must assume the responsibility of basic R&D in areas concerning national economic well being and security. I realize that the point of commercialization of any technology depends on more than the maturity of the technology alone. Risk, market and general economic conditions all play important roles, and therefore only private industries, through their own evaluations, can determine at what point they can commit to commercialization.

My opinion is that the federal government has been very effective in advancing technology, especially in the areas involving long range results and high risks. However, effective programs cannot be run without a steady federal commitment. Large funding fluctuations, including the continued threatening of early termination, prevent effective program planning and execution.

Let me conclude my testimony by quoting an article which appeared in the March 1982 Issue of Energy Insider (Vol. 5, No. 3). The article cited Deputy Secretary Davis: "Basic research is widely recognized to be needed for the nation's future economic development. But basic research does not command a high priority in private firms, particularly during times of high inflation and high interest rates. Industries such as electric and gas utilities that are subject to strict regulations aren't permitted the rates of return that would allow them to invest in developing advanced electric generation technologies such as magnetic fusion or breeder reactors." The MHD Program is such a developing technology, and the potential payoffs are worth seeing it through to completion.

Thank you very much for your attention.

Mr. FUQUA. Thank you very much, Dr. Wu.

Now, Mr. Dave Brown.

STATEMENT OF DAVE BROWN, PRESIDENT, BOARD OF DIRECTORS, MHD INDUSTRIAL FORUM, ACCOMPANIED BY JOHN D. CARNEY, DR. FRED WALTER, DR. ROBERT KESSLER, JACK SHERICK, BILL OWEN, AND BOB CUNNINGHAM

Mr. BROWN. Mr. Chairman and members of the committee, my name is Dave Brown. I am president of the board of directors of the MHD Industrial Forum located in Washington, D.C. The forum was established in September 1981, to focus Government and public attention on the tremendous energy and environmental benefits of the magnetohydrodynamic, MHD, technology. The forum represents a broad specturm of interests: industrial suppliers, research and development companies, individual scientists and scholars, and utilities anxious to put the technology to practical use.

I might add, Mr. Chairman, that I would like to submit my statement for the record in its entirety and will present just a brief summary.

In addition, I have with me a number of other people that would have liked to testify today if time had allowed. They are sitting in the front row to your left, the first two rows. If I might, Mr. John J. Carney is a senior vice president of the Edison Electric Institute; Dr. Fred Walter is a senior engineer specialist with the Montana Power Co.; Dr. Robert Kessler is vice president for the energy technology office of the AVCO-Everett Laboratory; and Mr. Jack Sherick is vice president and assistant general manager of Mountain States Energy, Inc. They have prepared statements that have been submited to the committee and we would like to have them included in the record. In addition, Mr. Bill Owen, the MHD program manager of Gilbert Associates; Mr. Bob Cunningham, the program development manager of the energy development group of TRW are also here to answer questions should the committee wish to ask such.

I am here on behalf of my industry, utility, and university membership to testify to the important role MHD power generation can play in the Nation's energy future. One of our gravest concerns is the lack of a consistent national energy policy and strategy. Since the 1973 energy crisis, three administrations have failed to develop a comprehensive strategy for this Nation's energy future.

Such a strategy must be a healthy component supporting the Nation's national security framework. Energy cannot in my opinion be divorced from our overall foreign and defense policies. It also is clear that fossil fuels will supply a substantial part of the energy requirements for several decades to come. Almost 50 percent of the electricity generated in the United States is currently derived from burning coal. Projections show that by the year 2000, not only will coal account for 60 percent of the total electrical generation, but because of the normal increase in electrical demand, the total electrical generation will have to double.

The doubling of the electrical generating capacity from coal will place a very serious burden on the electrical utility industry, the coal mining and coal handling industries and on the environment nationwide. In order to burn the large quantities of coal in an efficient and environmentally acceptable manner, utilities will need to turn to the newly developing advanced technologies. The Federal Government does the Nation a disservice when it proposes to terminate or curtail research programs that enhance the use of coal at a time when the regulated electrical industry predicts a struggle in meeting the projected demands in the next two decades.

The administration assumes that with the dissolution of DOE the development and demonstration of new technologies will be supported by private industry—and done so almost overnight. With the current rate structure imposed by the public utility commissions, the electric utility industry will find it impossible to absorb the added development cost and the cost of designing and constructing demonstration plants without the direct involvement of the Federal Government in many areas.

Magnetohydrodynamics, MHD, is a developing technology which has demonstrated a capability to provide the electric industry with an effective means of burning coal in the 1990's and into the next century. MHD is the only new technology which generates electricity directly from burning coal. Numerous Government and private industrial studies have shown that MHD offers the best combination of low cost of electricity, low pollution impact and high efficiency for base load utility electric power applications. These studies have the credentials of leading U.S. industrial companies and the results have not gone unnoticed by the electrical utilities. Although in general very conservative, the electric utility industry is actively considering MHD as an alternative to offset increasing fuel costs, to facilitate the substitution of coal for oil, to repower existing plants and to provide new central station powerplants.

The efficiency of an MHD powerplant can realistically be expected to be in the mid-40 to mid-50 percent range. Let me emphasize, this can be accomplished without any technological breakthroughs. This is in contrast to conventional coal burning powerplants in operation today which have an efficiency in the low 30-percent range. This means an MHD plant would require approximately 33 percent less coal than a similarly sized conventional coal fired powerplant. The benefits of requiring less coal in a powerplant go far beyond the obvious advantages of conserving a natural resource and the reduction of fuel costs.

A powerplant requiring less coal will produce less pollutants—well below projected EPA performance standards—per kilowatt generated including less carbon dioxide and less acid rain; there will be less solid and liquid waste to dispose of; and the plant will require less fresh water. The MHD technology promises improved environmental performance of coal plants, thereby facilitating licensing, increasing siting flexibility and expanding high-sulfur coal utilization. These significant advantages of MHD and others are being considered by the electrical utility industry and by equipment manufacturers.

Contrary to what you may have heard, there is strong utility support. The Edison Electric Institute has formed a task force on MHD composed of approximately 21 utilities and is presently formalizing the utility requirements for a commercial MHD plant. The Western Utilities Chief Executive Officers Conference, in January of this year, adopted a resolution strongly endorsing continued Federal R. & D. support for the MHD technology. In addition, the Electric Power Research Institute has contributed several million dollars to MHD development. Most recently, it has invested $2.9 million in an MHD inverter to be used at the CDIF. Utility support for commercialization grows at an increasing pace.

Base load electric generation plants are large and costly. It is not reasonable to expect that private industry will be able to assume the full financial burden required to bring to commercialization a technology such as MHD. The correct role of the Federal Government is to develop the technology base to a point where private industry will have the confidence to financially support a first commercial plant. Because of the critical role energy plays in our national economy and our national security, no reasonable technology should be terminated in its development cycle unless insurmountable problems are identified which would cast doubt on the technical merits or reliability of the technology.

In addition to the United States, I would point out several foreign countries have MHD development programs including Japan, Russia, China, Poland, India, and the Netherlands. In a recent visit to the United States a delegation of Japanese experts expressed interest in a cooperative program with the United States which could include an exchange of MHD test articles or direct financial participation of the Japanese in the MHD program in the United States. Discussions of several hundred million dollars in Japanese participation did not seem to deter their interest.

Finally, let me address the long-term funding requirements of the MHD program. DOE officials have correctly pointed out that funds required to bring the MHD program to the commercial market approximate $500 million. What they fail to point out is that only half, or $250 million, of that amount over the next 10 years will need to be Federal funds. If the U.S. program stabilizes in terms of Federal funding, substantial Japanese or other foreign funding in addition to industry, private funds are expected to produce the remaining $250 million. With a Federal investment to date of $420 million—over $150 million in Federal facilities and over $200 million in component development work—a cost benefit analysis of termination now would indicate the foolishness of not proceeding with development. I might also point out that while DOE management and other critics of MHD continually complain of the long-term costs of the MHD technology, they rarely address similar complete costs of other new coal-fired electric generating base load plant technologies. When such cost comparisons are made, they usually involve comparing the cost of components of other new systems to the cost of a complete base load MHD plant.

MHD is a coal conversion technology which should be viewed as essential to provide a high electrical energy productivity with cleaner and lower cost power generating plants which will be required in the 1990's and beyond. Although increased funding is a very sensitive matter at this time, the development of MHD should be continued and should receive a high national priority.

The MHD Industrial Forum strongly supports the inclusion of $40 million for the MHD program in the fiscal year 1983 DOE budget.

It is absolutely clear that priorities of the Department of Energy over the last 2 years have ignored the role that fossil energy will continue to play in the national energy picture and have ignored present or future prospects for technical progress of developing technologies. It is an ill-conceived approach and does not represent the best interests of the Nation's future. The resolution of our energy problems should be one of our highest national priorities. If energy, particularly fossil energy, technology development programs are terminated or reduced to the point where substantial technical progress cannot be made, the critically needed energy options or alternatives again will not be available to this country at the time of the next crisis—and we may not be as fortunate in the future as we were after the 1973 OPEC oil embargo.

Thank you, Mr. Chairman.

[The prepared statement of Mr. Brown follows:]

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COMMITTEE ON SCIENCE AND TECHNOLOGY

U.S. HOUSE OF REPRESENTATIVES

March 23, 1982

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Mr. Chairman and Members of the Committee, my name is Dave Brown. I am President of the Board of Directors of the MHD Industrial Forum with offices at 1725 DeSales Street, N.W. - Suite 800, Washington, D.C. 20036. The Forum was established in September, 1981, to focus government and public attention on the tremendous energy and environmental benefits of the magnetohydrodynamic (MHD) technology. The Forum represents a broad spectrum of interests: industrial suppliers, research and development companies, individual scientists and scholars, and utilities anxious to put the technology to practical use.

I am here on behalf of my industry, utility and university membership to testify to the important role MHD power generation can play in the Nation's energy future. One of our gravest concerns is the lack of a consistant national energy policy and strategy. Since the 1973 energy crisis, three Administrations have failed to develop a comprehensive strategy for this Nation's energy future. Such a strategy must be a healthy component supporting the Nation's national security framework. Energy cannot in my opinion be divorced from our overall foreign and defense policies. It also is clear that fossil fuels will supply a substantial part of the energy requirements for several decades to come. Almost 50 percent of the electricity generated in the United States (25 quads) is currently derived from burning coal. Projections show that by the year 2000, not only will coal account for 60 percent of the total electrical generation, but because of the normal increase in electrical demand, the total electrical generation will have to be increased to about 42 quads. Thus by the year 2000, the total electrical energy generated from the combustion of coal will have to double (from 12.5 quads to 25.2 quads).

Doubling of the electrical generating capacity from coal will place a very serious burden on the electrical utility industry, the coal mining and coal handling industries and on the environment nationwide. In order to burn the large quantities of coal in an efficient and environmentally acceptable manner, utilities will need to turn to the newly developing advanced technologies. The Federal Government does the Nation a disservice when it proposes to terminate or curtail research programs that enhance the use of coal at a time when the regulated electrical industry projects a struggle in meeting the projected demands of the next two decades.

For example, the DOE Energy Research Advisory Board (ERAB) concluded that electrical supply related programs should receive a low priority with a substantial reduction or termination in funding. Essentially all coal related programs that are directed toward the production of electrical energy were recommended for curtailment. These conclusions are in error. If this policy is adopted, the electrical industry will receive a crippling blow that will stagnate the industry at a time when great progress is being made to improve the efficiency and enviromental control of coal burning systems.

The Administration assumes that with the dissolution of DOE the development and demonstration of new technologies will be supported by private industry

— and done so almost overnight. With the current rate structures imposed by the Public Utility Commissions, the electric utility industry will find it impossible to absorb the added development cost and the cost of designing and constructing demonstration plants without the direct involvement of the Federal government.

Magnetohydrodynamics (MHD) is a developing technology which has demonstrated a capability to provide the electric industry with an effective means of burning coal in the 1990s and into the next century. MHD is the only new technology which generates electricity directly from burning coal. Numerous government and private industrial studies have shown that MHD offers the best combination of low cost of electricity, low pollution impact, and high efficiency for baseload utility electric power applications. These studies have the credentials of the leading U.S. industrial companies and the results have not gone unnoticed by the electrical utilities. Although in general very conservative, the electric utility industry is actively considering MHD as an alternative to offset increasing fuel costs, to facilitate the substitution of coal for oil, to repower existing plants and to provide new central station power plants.

The efficiency of an MHD power plant can realistically be expected to be in the mid-40 to mid-50 percent range. Let me emphasize, this can be accomplished without any technological breakthroughs. This is in contrast to conventional coal burning power plants in operation today which have an efficiency in the low 30 percent range. This means an MHD plant would require approximately 33 percent less coal than a similarly sized conventional coal fired power plant. The benefits of requiring less coal in a power plant go far beyond the obvious advantages of conserving a natural resource and the reduction in fuel costs. A power plant requiring less coal will produce less pollutants (well below projected EPA performance standards) per kilowatt generated including less carbon dioxide and less acid rain, there will be less solid and liquid waste to dispose of and the plant will require less fresh water. The MHD technology promises improved environmental performance of coal plants, thereby facilitating licensing, increasing siting flexibility and expanding high sulfur coal utilization. These significant advantages of MHD and others are being considered by the electrical utility industry and by equipment manufacturers. Contrary to what you may have heard, there is strong utility support. The Edison Electric Institute (EEI) has formed a task force on MHD composed of approximately 21 utilities and is presently formalizing the utility requirements for a commercial MHD plant. The Western Utilities Chief Executive Officers Conference, in January of this year, adopted a resolution strongly endorsing continued Federal R&D support for MHD technology. In addition, the Electric Power Research Institute (EPRI) has contributed several million dollars to MHD development. Most recently, it has invested 2.9 million dollars in an MHD inverter to be used at the CDIF. Virtually every utility in the country is following the progress of MHD development because of its economic and environmental advantages. Their support for commercialization grows at an increasing pace.

The National MHD Program has been designed to establish the technology base from which commercialization of this process can eventually be performed by private industry. Two government funded test facilities (the Component Development and Integration Facility (CDIF) and the Coal Fired Flow Facility (CFFF)) have been constructed, and, with diagnostic instrumentation development support from Mississippi State University, have started to provide test results which confirm the theoretical analyses. Information from these facilities is vital to the decision by the electrical utilities to financially support the construction of commercial MHD plants. The reduction or curtailment of Federal funds supporting this technology at this crucial time in the development of MHD is not in the best interests of the country or the private sector. There have been no reports written by private industry which recommend the termination of this development program. Termination of Federal support now will shake industry investment in the technology.

Base load electric generating plants are large and costly. It is not reasonable to expect that private industry will be able to assume the full financial burden required to bring to commercialization a technology such as MHD. The correct role of the Federal Government is to develop the technology base to a point where private industry will have the confidence to financially support a first commercial plant. Because of the critical role energy plays in our national economy and our national security, no reasonable technology should be terminated in its development cycle unless insurmountable problems are identified which would cast doubt on the technical merits or reliability of the technology. To date there are no technology barriers that would prevent the development of MHD to commercialization. Nor has the engineering community had to revise its performance predictions. Ten years ago it was predicted that an MHD plant could achieve efficiencies approaching 50 percent. As development testing over the years provided improved engineering data, the predicated 50 percent efficiency has remained valid and even higher efficiencies appear possible.

During the past year, major milestones in technology development have been achieved. Coal combustor development work has demonstrated design feasibility with good performance of critical combustor parameters such as conductivity, slag rejection, heat loss, and high carbon conversion efficiencies. Tests have been conducted at the TRW facility utilizing a 20 megawatt combustor to verify performance and reliability over extended duration. Channel durability and electrode lifetime are critical issues in the development and commercial acceptance of MHD. The recently completed 1,000 hour duration test at Avco, which simulated coal-fired MHD channel operation by injecting fly ash and sulfur into an oil burner, extrapolated to a channel lifetime of 8,000 hours. Efficient superconducting magnets for use in the MHD process have been successfully demonstrated with the performance of a 40 ton, 5 Tesla superconducting magnet shipped to the USSR as part of the cooperative program and more recently by the completion and initial checkout of a 200 ton, 6 Tesla magnet built by the Argonne National Laboratory. Preliminary checkout of the 6 Tesla magnet showed the magnet exceeded design specification by producing a 6.2 Tesla field. In addition, major test facilities in the program have begun producing needed engineering data. These facilities and test data include heat and seed recovery and environmental performance data from the recently activated Coal Fired Flow Facility (CFFF) in Tullahoma, Tennessee, high energy extraction performance data from the High Performance Demonstration Facility at the Arnold Engineering Development Center, and 50 megawatt MHD power train performance data from the recently activated Component Development and Integration Facility (CDIF) in Butte, Montana.

In addition to the United States, several foreign countries have MHD development programs including Japan, Russia, China, Poland, India, and the Netherlands. By far the most ambitious programs are those in Japan and the USSR. The Japanese have a 15 megawatt oil-fired test facility at the Electro Technical Laboratory in Takasago. This facility includes both the MHD topping and steam bottoming cycles together in a single facility. The Soviets have a major test facility at the High Temperature Institute in Moscow. This is a 250 megawatt gas-fired plant with both topping and bottoming cycle components utilizing a low-field conventional magnet and has associated with it a smaller 25 megawatt thermal bypass loop which utilizes a U.S. supplied superconducting magnet. Furthermore, the USSR is designing a commercial scale, 500 megawatt gas-fired MHD power plant which should be completed by 1986 and is planning a commercial scale, coal-fired unit some time after 1990.

In a recent visit to the United States, a delegation of Japanese experts expressed interest in a cooperative program with the United States which could include an exchange of MHD test articles or direct financial participation of the Japanese in the MHD program in the U.S. Discussions of several hundred million dollars in Japanese participation did not deter their interest.

The development of any technology to the point of commercialization, is complicated by the number of steps required. This is particularly true for a technology being developed for the electric utility industry which requires a very reliable system, and therefore, it is essential that each new system be developed to a high degree of perfection before it is considered commercial. The existence of the MHD Industrial Forum, the EEI MHD Task Force, EPRI's private funding to date, the Western Utilities Chief Executive Officers resolution and others all attest to major industrial interest and participation.

The technology is just now entering a phase where major private or foreign investment will soon occur. But investments will not be made unless Federal support for MHD continues. To date the Federal Government's role has been the development of a technology data base and providing test facilities which have just begun operations for performance demonstration. Private industry has been involved in establishing design requirements and providing equipment and engineering services as contractors to the government. As demonstration testing proceeds, it is fully expectd that the role of private industry will shift from contractor to that of investor. The next step in the path to commercialization will probably be a small size commercial demonstration plant which will have a substantial financial commitment from the private and foreign sectors. The demonstration plant will then be followed by commercial power plants which will be funded completely by private industry. The commercialization of MHD will have very substantial financial participation by private industry, however, it is understandably premature at this juncture of the commercialization process. This applies to any advanced technology at this research and development stage.

Finally, the long term funding requirements of the MHD program must be addressed. DOE officials have correctly pointed out that funds required to bring the MHD program to the commercial market approximate $500 million. What they fail to point out is that only one half, or $250 million, of that amount over the next ten years will need to be Federal funds. If the U.S. program stabilizes in terms of Federal funding, substantial Japanese or other foreign funding in addition to industry, private and SFC funds are expected to produce the remaining $250 million dollars. With a Federal investment to date of $420 million (over $150 million in federal facilities and over $200 million in component development work), a cost benefit analysis of termination now would indicate the foolishness of not proceeding with development. I might also point out that while DOE management and other critics of MHD continually complain of the long term costs of the MHD technology, they rarely address similar complete cost of other new coal fired electric generating baseload plant technologies. When such cost comparisons are made, they usually involve comparing the cost of components of other new systems to the cost of a complete baseload MHD plant.

MHD is a coal conversion technology which should be viewed as essential to provide a high electrical energy productivity with cleaner and lower cost power generating plants which will be required in the 1990s and beyond.

Although increased funding is a very sensitive matter at this time, the development of MHD should be continued and should receive a high national priority.

It is absolutely clear that priorities of the Department of Energy over the last two years have ignored the role that fossil energy will continue to play in any National energy picture and have ignored present or future prospects for technical progress of developing technologies. It is an ill conceived approach and does not represent the best interests of the Nation's future. The resolution of our energy problems should be one of our highest National priorities. If energy (particularly fossil energy) technology development programs are terminated or reduced to the point where substantial technical progress cannot be made, the critically needed energy options or alternatives again will not be available to this country at the time of the next crisis -- and we may not be as fortunate in the future as we were after the 1973 OPEC oil embargo.

Mr. FUQUA. Thank you, Mr. Brown.

Mr. Yeager, the DOE has proposed $2.5 million for the Grimethorpe project. What type of information will this funding purchase for us, and with the reduced funding level is it going to result in a sacrifice of technical information?

Mr. YEAGER. It will certainly result in a sacrifice of technical information in the United States. This is, as you are aware, a program which is funded by the United States as well as the United Kingdom and West Germany. The funding reduction on the part of the DOE signifies a phasing-out of support by the United States for this key facility at a time when it is just beginning to be operational. We have the sunk costs that the Government has put into the construction, and now we are phasing out as we are beginning to get the operational results. I think that is unfortunate.

Mr. FUQUA. Mr. Spencer, on page 7 of your testimony you talked about the various projects in MHD, and I gathered that you said, in effect, that you favored the Montana project. Is that your position or the position of EPRI?

Mr. SPENCER. Mr. Fuqua, that is not only my position, but that is the position of EPRI, we think that the component development integration facility definitely should be operated for a period of at least a couple of years in order to take advantage of the investment that has been made by both DOE and EPRI.

Mr. GORE. Will the Chairman yield?

Mr. FUQUA. Yes.

Mr. GORE. I am not quite sure I understood the interchange. You didn't mean to say that you favored it to the exclusion of the other efforts in this field, did you?

Mr. **Spencer**. No, I said we felt that was the No. 1 priority and perhaps other related test facilities should continue to be operated in order to make a total meaningful MHD effort.

Mr. **Gore**. I will pursue that on my own time.

Thank you.

Mr. **Fuqua**. Dr. Wu, how many students are working at the coal-fired flow facility?

Dr. **Wu**. It varies from year to year. Prior to the reduction we had 23 students working on the program.

Mr. **Fuqua**. How many?

Dr. **Wu**. We had 23 last year, but now we have only 9 because of the decrease in funding.

Mr. **Fuqua**. Those that get degrees in MHD, where are they placed?

Dr. **Wu**. In various organizations. In fact, they are well sought by industries because the MHD provides them a very good sound foundation into many high technology areas. I don't have enough students to supply for the industry.

Mr. **Fuqua**. Mr. Brown, MHD is a technology that needs a balanced program of basic and applied and engineering development by industry, but we are in some tight budget times right now and which of these activities do you think are the most important, and would your organization—the task force that you mentioned, I assume—try to establish some priorities of what needs to be done at this time in MHD?

Mr. **Brown**. I think the best answer to that, Mr. Chairman, is that the technology base itself needs to be completed. That, in the best estimates I think of most of the people in the field, would take another 3 to 4 years before you have that technology base substantially complete.

That optimally on a minimum budget would be in the area of $35 to $40 million for the next 3 or 4 years.

Mr. **Fuqua**. How much is industry willing to put up?

Mr. **Brown**. I think this is a situation, Mr. Chairman, where you have a technology that shows great promise and you have great interest, but with both from a utility perspective of the unallowable cost aspect of public service commissions in terms of putting that kind of capital into a rate base, as well as industry's backing off from the R. & D. picture substantially over the last two decades makes it difficult for industry to contribute significant dollars. Now, they have contributed a substantial amount over the years.

Mr. **Fuqua**. Secretary Mares was up last Thursday and I offered to give him an A for effort in supporting the administration. And he said—maybe you were here—that industry would pick that up and utilities could pick that up.

Mr. **Brown**. I think, Mr. Mares erred in that statement, Mr. Chairman, not only in MHD but in a number of advanced technologies in the fossil fuel area. The Electric Power Research Institute, as an R. & D. arm of the electric utility industry, is spending substantial amounts of dollars in a number of areas, most of them at a near commercial stage or ready to go commercial. In the case of MHD and a number of other technologies like fuel cells, combined cycle, et cetera, we are talking apples and oranges comparison and none of them are near that commercial stage yet. They are a few years away, but they all offer important benefits.

Mr. FUQUA. I was going to ask you about the program. If something pays off in less than 15 years is it going to be pursued by the private sector?

Mr. SPENCER. I personally feel, as Dave said very well, that there are other more important coal utilization priorities for the utility industry at this point in time; those discussed by Mr. Yeager and myself.

Mr. FUQUA. Are you thinking now of a more immediate range or long range?

Mr. SPENCER. Essentially investments over the next 5 or 6 years, and technologies which we think then would be commercialized in the early 1990's. And, therefore, although we continue to agree that MHD has a lot of promise we think the private sector funding for it will be very minimal because of the competition for R. & D. dollars for coal-fired plants or gasification combined cycle. So that I do not see the power industry picking up a significant part of the MHD R. & D. program.

Mr. FUQUA. Thank you.

Mr. Gore.

Mr. GORE. Thank you very much, Mr. Chairman. I appreciate the opportunity to ask questions here today, and I would ask unanimous consent that Dr. Wu's entire statement appear in the record, and the statements referred to by David Brown that were submitted to the committee, if the subcommittee has no problem with that.

Mr. FUQUA. We have already done that.

Mr. GORE. Thank you very much.

In response to your comments about industry picking up the tab for this program, Mr. Chairman, I would like to focus the subcommittee's attention once again on the statement that Dr. Wu highlighted at the conclusion of her statement, and it comes from Deputy Secretary of Energy Davis, and I quote:

> Basic research is widely recognized to be needed for the Nation's future economic development, but basic research does not command a high priority in private firms, particularly during times of high inflation and high interest rates. Industry such as electric and gas utilities that are subject to strict regulations aren't permitted the rates of return that would allow them to invest in developing advanced electric generation technologies.

Now, I think that that summarizes the situation very well. The administration at times appears to be pretending that the energy crisis is over with in spite of the very painful experience this Nation has had swinging like a pendulum between periods of extreme urgency and crisis at one time and then periods of false calm and relaxation at another time. We are now in a period of false calm and relaxation as well as a period of intense pressure on the budget, including the budget for research and development, and it would be tragic for this country to abandon its efforts to make its fossil fuel base more productive and to develop the technologies that can substitute for imported oil. So I think that it is a very unfortunate turn of events.

Now, Dr. Wu—to explore the potential of MHD—just exactly how great is the increase in efficiency of MHD over conventional coal plants?

Dr. **Wu**. In my recent statement I have cited that from many studies we have done, and many people from the industry, have shown that in the early stage a 30-percent increase is possible.

Mr. **Gore**. Thirty percent?

Dr. **Wu**. For the early stage.

However, for mature plants a 50-percent increase is expected.

Mr. **Gore**. A 30-percent increase in efficiency for an early plant and 50 percent for a matured plant?

Dr. **Wu**. Yes; over the conventional steam power plant.

Mr. **Gore**. Now, what about the simultaneous improvement in pollution control. What is the advantage in pollution control of MHD?

Dr. **Wu**. Well, the MHD process itself inherently has a cleaning process built in. As a consequence there is no need for a scrubbing system at all to remove sulfur contents in the emissions. Also at the same time the MHD process, itself, will operate much better if it is operated oxygen deficient in the MHD cycle. This is exactly the principle for staged combustion for reduced NO_x. We have demonstrated experimentally this is indeed true, we can reduce NO_x levels to very very low levels, approximately one-seventh of what the EPA standard is.

Mr. **Gore**. This is a very important point that I want to explore next, and I would invite the attention of my colleagues to this point. If we get to the stage in the budget crunch that all three of the facilities cannot continue to be pursued—up until now we have talked about all three of them being pursued; that's my position. Evidently that is not EPRI's position this morning. But if we get to the stage that EPRI evidently anticipates, and we can pursue part of the MHD effort—one of the efforts assumes the scaling up to the demonstration plant—but if I understand your testimony, Dr. Wu, there are a large number of spinoff technologies and discoveries associated with the work at UTSI that can be applied across the board to other fossil fuel technologies, is that correct?

Dr. **Wu**. That is correct.

Mr. **Gore**. The Department of Energy has gone to great lengths to discount the value of MHD in the absence of a proof-of-concept plant. In your opinion, is such a plant necessary to the validity or the application of the project data? And, if your response is yes, could you give us some examples?

Dr. **Wu**. I believe that the MHD technology has a very broad application in our areas concerning high-temperature coal combustion; therefore, the data itself is very valuable in many different areas, wherever it is related with high-temperature technologies. Whether we will get into the later stage of proof of concept or not really doesn't matter to the value of the data, because the data is valuable, in terms of Mr. Yeager, for instance, who said earlier "the materials problems." Of course, MHD has the same problem. The results we will obtain from the materials problems can be applied into his area, the whole high sulfur coal combustion and many other areas can be applicable. I believe the technology itself has sufficient justification to be pursued on its own merit.

Mr. **Gore**. Thank you.

And in conclusion, Mr. Chairman, let me just say that my position is unwavering in support of the MHD technology. I think this country would be shortsighted to halt its progress in this area, and I support the CDIF facility as well. I think it would be tragic in the extreme to halt our work at a bare-bones minimum. We must continue to acquire and evaluate the scientific data that is coming out of this program, because it has such teriffic promise.

Thank you very much, Mr. Chairman.

Mr. **Fuqua**. Mr. Fish.

Mr. **Fish**. Mr. Chairman, I would like to yield my time to the gentlewoman from Massachusetts, Mrs. Heckler.

Mrs. **Heckler**. I thank my colleague for yielding, and I thank the chairman of the committee for allowing me to sit in on this subcommittee of which I am not a member but in which I have such a great interest.

Several years ago the distinguished former president of Boston Edison said that the MHD technology was the most promising for energy generation in the future. I feel that the development of this technology is absolutely essential and I do have some specific questions on the issue.

I would like to direct my first question to the representative from AVCO.

Would you please join the witness table.

As I understand it, AVCO-Everett Research Laboratory has the most extensive operational MHD test facilities in the United States. AVCO has been in the forefront of MHD development since performing the first successful demonstration of MHD power generation. I would like to know from AVCO's perspective and experience whether there are any major technical obstacles which would prevent MHD from becoming a commercial reality?

Dr. **Kessler**. In my opinion there are no major technical obstacles or insurmountable barriers that would prevent it from becoming a commercial reality. What is required is development to proof of concept.

Mrs. **Heckler**. Well, it is also my understanding that earlier AVCO had originated the development of this technology, that you were visited by Russian guests who inspected your facilities and required that as one of the basic elements of their visit to the United States, and that they thereupon returning to the Soviet Union developed this technology and there is a plant in operation in the Soviet Union. Is that true, and could you elaborate on those facts?

Dr. **Kessler**. There was a high-level Soviet delegation that visited AVCO in the early sixties to inspect the MHD facilities. They did go home very interested and started their own MHD program, and now there is a very substantial size Russian effort in this field which involves a large size plant in Moscow and a commercial size plant, which is now being designed and for which ground has been broken, in a place called Riza about 150 kilometers from Moscow. And they—the Russians that is—intend to have this plant, this commercial-sized MHD plant built and in operation by the middle eighties, 1985–86.

Mrs. **Heckler**. What I would like to have Mr. Brown restate for the record is the answer to the perennnial question in the Congress about funding for MHD which is: If the technology is so promising why is it that the private sector or the utilities are not funding it?

Mr. **Brown**. I think, Mrs. Heckler, the key, as I indicated before, is that the utilities are basically hamstrung in their ability to spend funds in an R. & D. area. Their contributions to EPRI are the most significant thing they are able to do primarily because public utility commissions in every State regulate the amount of capital cost, plant construction, and new equipment, et cetera, that is allowed into the rate base. The consumer must pay those bills. The commissions have generally been very protective about how that electric rate goes up.

On the industrial side, I should point out that industry has made substantial contribution to MHD over time, probably $30 to $40 million in the last 10 years, including EPRI's contributions, the R. & D. capability of most of the Nation's industry is not what it once was. There are a lot of things that have contributed to that, including a substantial Federal initiative in many R. & D. areas which has inhibited the ability of industry to input into that process. It doesn't mean they have cut out R. & D. It means it is substantially less and that no single entity can handle the size of the cost of a technology like MHD.

Mrs. **Heckler**. I would like to ask any member of the panel who wishes to respond about an evaluation of the Department of Energy's Energy Research Advisory Board report which recommended that MHD development should be terminated because, quote,

> There are other technologies such as combined cycle gas turbines available for increasing the efficiency of large central power generation stations that do not have the extreme temperature erosion problems associated with the use of coal in MHD.

Dr. **Kessler**. I would like to respond to that.

I believe this statement was quite misleading. There are other technologies but they do not offer as high an efficiency potential as MHD, and this has been confirmed in a number of studies, including the NASA-sponsored DCAS study of 1976 and the EPRI study of 1978 which were performed by such companies as General Electric and Westinghouse.

Second, with respect to the "extreme temperature erosion problems," I believe this refers to the electrode erosion problem which is most commonly quoted, and I believe that our recent 1,000-hour electrode test in which there was a minimal erosion and in which the extrapolated lifetime as 7,000 to 8,000 hours more than adequately answers that criticism.

Mr. **Brown**. Mrs. Heckler, may I also answer that question just briefly?

And that is to point out that I don't believe anybody in the MHD field, or in any other fields, believe that it is a horse race to see, in the competitive sense, who is going to get there first. The real point is that it is an apples and oranges comparison, that all of the technologies in this area should advance and they should be brought to the point where a commercial decision can be made. That, I think, is one of the great failures of DOE over time, and its winnowing process has now brought it down to roughly four technologies in this advanced technology category, and that it is not a horse race, each should proceed to the point where it is apparent what that technology's contribution might be. And I think that is an important point to remember.

Dr. **Wu**. Mrs. Heckler, can I also substantiate this one point to supplement my colleagues' discussion on this issue?

The efficiency of any cycle depends on its temperature. The higher the temperature the higher the efficiency. Certainly at higher temperature there will be more problems. However, we were able to solve some of the problems with significant advancements having been made, particularly in the electrode area. I do not believe these problems are insurmountable—that we cannot solve.

Mr. **Spencer**. Can I just add one thing, Mrs. Heckler since I am responsible for both coal gasification combined cycle technology and MHD at EPRI. I would simply say that our judgment is that coal gasification combined cycle plants will mature earlier than MHD and will give us the opportunity to reach the high thirties and perhaps low forties in overall efficiency producing electricity from coal; that if MHD is successful it should be able to attain at least the midforties and perhaps low fifties. I am not quite as optimistic as our colleagues, but certainly it will show an improvement in overall efficiency vis-a-vis coal gasification combined cycle. So I don't think it is an either/or situation, it is a matter of priority, and our priority is first to get the efficiency into the high thirties and low forties and then later to improve beyond that.

Mrs. **Heckler**. Yes.

Dr. **Kessler**. If I could make one final comment with respect to the erosion problem, I have a report on the 1,000-hour electrode test with some very impressive pictures of the electrode condition at the end of the test which I would like to offer to the committee.

Mr. **Fuqua**. Thank you.

Thank you, Mrs. Heckler.

Mr. Winn.

Mr. **Winn**. Mr. Chairman, I will not take my time. I want to commend you for holding these hearings, and since Mr. Fish had yielded to Mrs. Heckler I will yield to Mr. Fish.

Mr. **Fuqua**. Mr. Fish, it is your turn. If it was Mrs. Heckler's turn I would assume she would yield to you.

Mr. **Fish**. Mr. Yeager, it would appear that with EPRI's emphasis in short-term development, the Department of Energy's decision to concentrate on long-range research would be quite beneficial. Do you think this is a wise approach? If you could, comment on possible cooperative efforts and challenges.

Mr. **Yeager**. I think that it is a practical basis for cooperation. I think the concern is that the funding base for the long-range, high-risk research is more illusory than real. As you look at the budget there is just not very much substance in terms of what I would call the higher risk areas, as a result we are ending up with a net sacrifice of that effort.

The areas where we have been most active in cooperative direct coal combustion ventures with the Department of Energy have been primarily in fluid bed technology. As I mentioned in my testimony, the one area that gives me the greatest concern is the pressurized fluidized bed area. The industry has gone from skepticism on the technology to the point where it considers it a very high-potential technology for the early nineties. The industry is prepared, and we have funding in our budget, for the construction of a prototype commercial facility. Key to being able to make any sensible commitment of that sort, however, is the successful completion of the development projects that are under way by DOE, specifically the Grimethorpe project in England and the Curtiss-Wright combined cycle plant. If those projects are canceled, as the budget would indicate, then I think we have also essentially canceled the private sector commitment because there is no way that we can fill that gap.

Mrs. **Heckler**. Mr. Chairman, I would like to ask my colleague to yield.

Mr. **Fish**. I would be happy to yield.

Mrs. **Heckler**. I would just like to clarify, for my own understanding, one point that I think is terribly significant in terms of the consideration of environmental issues in the Congress. I would like to have any one of the panel who wishes to respond discuss the environmental questions and the environmental assets and costs to the environment of the alternative technologies being suggested. That would be MHD versus the combined cycle gas turbine, or other technologies. Would anyone like to respond?

Dr. **Wu**. I can say something about MHD power because at the moment the pollution control for MHD does not cost any additional money. It is a part of the plant itself. So there is no added cost at all.

Mrs. **Heckler**. What would be the cost of pollution control for the other technologies? Is there any estimate available?

Mr. **Spencer**. Yes.

For gasification combined cycle plants the cost of high amounts of sulfur removal and meeting very low NO_x and particulate emissions is rather minimal, also. It would add probably $30 per kilowatt to a plant that costs about $1,000 per kilowatt. So the advantage of both MHD and integrated gasification combined cycles is that the cost for sulfur removal and meeting very tight NO_x and particulate standards is relatively straightforward as compared to direct coal-burning processes.

Mr. **Yeager**. If I may just amplify on that point. In present conventional coal-fired powerplants, to meet the EPA's national standards, invest about 40 percent of their capital cost in environmental controls. In all of the technologies we are talking about-the key driving force by the industry is to eliminate, or at least minimize, this very parasitic cost factor on the user of electricity. Fluid-bed technology also eliminates scrubbers and much of the other post-combustion air pollution control technology. It also simplifies the waste disposal problem by providing a dry byproduct rather than a wet sludge byproduct. In addition, it provides the opportunity to burn a much wider range of fuels, thus being able to accommodate high-sulfur coal, low-sulfur coal, and municipal refuse with a single design.

Mrs. **Heckler**. If my colleague would yield further?

Mr. **Fish**. Certainly.

Mrs. **Heckler**. I would like to direct a question to the AVCO representative. I understand that you have had a recent test of the life of the 8,000-hour channel and that this has met the utility industry requirements for early commercial powerplants. Would you explain that in some detail?

Dr. **Kessler**. In order to be accepted it has to be something on the order of a year or 8,000 to 9,000 hours. But for early commercial plants, studies have shown—studies performed by companies such as Westinghouse, some other MHD outfits which have been involved in this kind of study—have shown that 2,000 to 3,000 hours is probably acceptable for the first early commercial MHD plants, and so that our 1,000-hour test, we think, is well on the way toward that.

Mrs. **Heckler**. I thank my colleague for yielding.

Mr. **Fish**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Mr. Volkmer.

Mr. **Volkmer**. Along those same lines, I would like for you to compare with direct combustion, the utilization of water and the necessity to utilize water with MHD and gasification and the combined cycles. How much water is necessary?

Mr. **Spencer**. Let me talk about integrated gasification combined cycles—Dr. Wu would like to talk about MHD.

In the configuration which we have at coolwater, we will be using about 60 percent of the cooling water requirement that you would have for a normal coal-fired plant of that same capacity. However, we have carried out economic evaluations which indicate we could go to as low as 10 percent of the water consumption for a conventional coal-fired plant by using dry cooling towers and still only have a very modest increase in overall plant cost.

Mr. **Volkmer**. Dr. Wu?

Dr. **Wu**. The water requirement of the MHD combined cycle essentially is the downstream part, the steam powerplant which essentially would be no different from a conventional powerplant except that overall powerplant has much higher efficiency. Relatively speaking, the requirement for water would be greatly reduced.

Mr. **Volkmer**. That is just as the use of coal would be?

Dr. **Wu**. We do not need any additional water. The water required for the MHD part is completely self-contained. It is a closed loop.

Mr. **Volkmer**. Mr. Yeager, I would like to ask you or Mr. Spencer, either one, have you—I am sorry I missed your testimony on another area and that is the closing down of the H-coal and EDS plants. We heard testimony last week about proceeding with further testing at Wilsonville and I would like to have your comments on this.

Mr. **Spencer**. Congressman, I will comment on that. It is included with my testimony. Our technical judgment is that we should continue operating the Wilsonville test facility for another 3 or 4 years. It is a relatively modest annual investment of about $12 million, of which EPRI has been expending about one-third and the Government two-thirds. So, we do see a value in continuing that research.

Mr. **Volkmer**. Will EPRI continue to fund at those levels?

Mr. **Spencer**. Yes; we do anticipate funding at nominally the $4 million to $5 million level. However, we will not be able to accept the total operating costs of $10 million to $12 million.

On the H-coal and EDS pilot plants, our technical judgment is that we need another full year of operation to assure that we have established an adequate design basis on each one of those two process technologies on a variety of coals. Therefore, I have recommended in my testimony that those pilot plants be retained through fiscal year 1983 if at all possible with a DOE commitment of approximately $60 million. I believe that would then bring in enough private sector funding so that we could operate the two pilot plants for another year.

Mr. **Volkmer**. I would like to continue, then, on with the pressurized fluidized bed, and I believe you mentioned, Mr. Yeager, that you foresee the Grimethorpe operation as concluding our participation in that?

Mr. **Yeager**. The DOE plan, as I understand it, will maintain a scaled-down level of support in fiscal year 1983 at Grimethorpe, phasing out after that point. The key issue is that that plant is just beginning to operate, and provide useful data. To cancel participation after the initial year of operation will leave unanswered the majority of the objectives for which the U.S. investment in that plant was originally defined.

Mr. **Volkmer**. Can we foresee any private investment in pressurized fluidized bed?

Mr. **Yeager**. Yes, sir. In fact, right now the Electric Power Research Institute is investing about $5 million a year in the basic technology, particularly resolving the hot gas cleanup issue and related turbine reliability. This is being done in cooperation with the Grimethorpe and Curtiss-Wright projects. If those projects are lost the vehicle to confirm the results we have been developing will also be lost.

Second, as I mentioned, we plan, based on the successful conclusion of the Grimethorpe and Curtiss-Wright projects, to proceed with the construction of a prototype commercial PFBC facility with the utility industry. That would be funded in the private sector.

Mr. **Volkmer**. You are looking at about what year for that?

Mr. **Yeager**. Well we are anticipating, if these other programs stay on schedule, about 1985 for that kind of a program commitment to begin. This initial demonstration would be completed by the end of the decade.

Mr. **Volkmer**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, Mr. Volkmer, and we want to thank our panel for being here this morning. We appreciate very much the statements and thank you for being here.

Our next panel, Mr. Arnold Kossar, vice president of engineering at Curtiss-Wright Corp.; Dr. George Mechlin, vice president and director of research, Westinghouse Electric Corp., Mr. Robert Ross, the director of engineering, Seminole Electric Cooperative will testify on behalf of the Fuel Cell Users Group; and Mr. Eric Reichl, who after many years in industry is a private consultant.

We will start with Mr. Kossar.

If you have statements, we will make them in their entirety part of the record as I have mentioned to the previous panel. And if you want to summarize, certainly feel free to do so.

Mr. **Kossar**. Thank you, sir.

STATEMENT OF ARNOLD F. KOSSAR, VICE PRESIDENT, ENGINEERING, CURTISS-WRIGHT CORP.

Mr. **Kossar**. I won't go into the attachments and addendum to my statement. I would like to read the core portion of it.

My name is Arnold F. Kossar and I am vice president of engineering for Curtiss-Wright Corp., and my testimony today centers on the pressurized fluidized bed pilot plant, a joint venture development between the Department of Energy and Curtiss-Wright Corp. calling for the testing, in an integrated combined cycle pilot plant, of pressurized fluidized bed technology for the combustion to coal. This process will improve the efficiency of coal-burning techniques at a considerable savings both economically and environmentally.

As background, the contract was entered into between Curtiss-Wright and the predecessor agency of the Department of Energy in 1976. The contract was basically R. & D. in nature and was predicated on a two-thirds Government/one-third private support. To date, the Government has appropriated $58 million for the project. The project plan was originally established in four phases—preliminary engineering, pilot plant design, pilot plant construction, the third phase we are in now, and pilot plant operation. The Government's overall plan was to follow the pilot plant program with the construction of a full utility—scale modular demonstration plant. Phase 3, pilot plant construction, should be completed in 1983. The Congress appropriated $15 million for fiscal year 1982. As of March 1, the Department of Energy has withheld $10.8 million of this amount pending a request for a reduced cost to the Government for the overall project. Curtiss-Wright has presented its reduced cost plan to the Department of Energy.

It is our hope that as a result of the expression by the Congress the Department of Energy will release the $10.8 million and that an appropriation in fiscal 1983, not of the approximately $9 million contemplated, but of $5.5 million, will be forthcoming to finalize the phase 3 construction of the pilot plant. This is a saving of $3.5 million on the construction alone. After completion of construction, the plant will run for a 2-year period.

The cost to the Government of the phase 4 operation has been projected as approximately at $11.6 million. We have advised DOE that at this point we believe it can be reduced to $1.5 million per year for 1984 and 1985, and we have hopes that we will be able to reduce that figure to zero. We simply have not had time to do that as yet. Thus, we are asking this committee's support to cause the Department of Energy to release the $10.8 million being withheld and to appropriate the $5.5 million to finish the pilot plant. Balanced against this is an estimated termination cost of $15 million and a loss of almost $58 million that the Government will have invested in this project. A preliminary breakdown of the termination cost estimate is one of the attachments.

The project has been technically successful to date. In addition, a number of independent economic studies have confirmed reductions for electric utilities in both capital and operating cost in the range of 10 to 20 percent when PFB plants are compared to modern pulverized coal-fired plants equipped with stack gas scrubbers. As a result, the Electric Power Research Institute has strongly endorsed the completion of construction of the pilot plant and indicated its interest in participating financially and in the testing during phase 4. EPRI has pointed to the pilot plant as the only facility in the world capable of turbine testing necessary to permit the projection of actual commercial equipment. Similarly, a number of utilities including Public Service Electric and Gas Co. and the Power Authority of the State of New York have promised support during phase 4 testing. Even our architects and engineers are so committed to the future of the program that they are willing to forego their fees.

The addendum to this statement expands on the above points.

Thus, it would appear absolutely imprudent to terminate a project that is so close to completion and now of so little cost to the Government. Additionally, we should point out that in the original plan the Government had anticipated providing 50 percent of the funding of the demonstration plant. Curtiss-Wright has indicated to the Government that it will proceed without such Government involvement. Obviously, this will be done in conjunction with a utility which has yet to be firmly identified.

We sincerely believe that the economics, maximum energy utilization potential, and superior environmental performance of this technology dictate the continuation of the project.

We thank you very much for the opportunity to appear before the committee. We, of course, would be happy to respond to any questions you may have now or at some later date.

Thank you, sir.

Mr. FUQUA. Thank you, Mr. Kossar.

[The prepared statement of Mr. Kossar follows:]

March 23, 1982

House Committee on Science and Technology
Subcommittee on Energy Development and Applications
Statement of A. F. Kossar, Vice President - Engineering
Curtiss-Wright Corporation

Mr. Chairman and Members of the Subcommittee, my name is Arnold F. Kossar and I am Vice President - Engineering of the Curtiss-Wright Corporation. I am accompanied today by our Washington, D. C. counsel, William F. Ragan, from the law firm of Ragan & Mason. My testimony today centers on the Pressurized Fluidized Bed Pilot Plant, a joint venture development project between the Department of Energy and Curtiss-Wright Corporation calling for the testing, in an integrated combined cycle pilot plant, of pressurized fluidized bed technology for the combustion of coal. This process will improve the efficiency of coal-burning techniques at a considerable saving both economically and environmentally.

A contract was entered into between Curtiss-Wright and the predecessor agency of the Department of Energy in 1976. The contract was basically R&D in nature, and was predicated on two-thirds government/one-third private support. To date, the government has appropriated $58 million for the project. The project plan was originally established in four phases -- preliminary engineering, pilot plant design, pilot plant construction (the third phase we are in now), and pilot plant operation. The government's overall plan was to follow the Pilot Plant program with the construction of a full utility scale modular demonstration plant. Phase Three, pilot plant construction, should be completed in 1983. The Congress appropriated $15 million for FY 1982. As of March 1, the Department of Energy has withheld $10.8 million of this pending a request for a reduced cost to the government for the overall project. Curtiss-Wright has presented its reduced cost plan to the Department of Energy.

It is our hope that as a result of expression by the Congress, the Department of Energy will release the $10.8 million and that an appropriation in 1983, not of the approximately $9 million contemplated, but of $5.5 million, will be forthcoming to finalize the Phase Three construction of the pilot plant. This is a saving of $3.5 million on the construction alone. After completion of construction, the plant will run for a two-year test period. The cost to the government of the Phase Four operation had been projected as approximately $11.6 million. We have advised DOE that at this point we believe it can be reduced to $1.5 million per year for 1984 and 1985, and we have hopes that we will be able to reduce that figure to zero. We simply have not had time to do that as yet. Thus, we are asking this Committee's support to cause the Department of Energy to release the $10.8 million being withheld and to appropriate the $5.5 million to finish the pilot plant. Balanced against this is an estimated termination cost of $15 million and a loss of almost $58 million that the government will have invested in this project. A preliminary breakdown of the termination cost estimate is attached.

The project has been technically successful to date. In addition, a number of independent economic studies have confirmed reductions for electric utilities in both capital and operating cost in the range of 10 to 20% when PFB plants are compared to modern pulverized coal fired plants equipped with stack gas scrubbers. As a result, the Electric Power Research Institute has strongly endorsed the completion of construction of the pilot plant and indicated its interest in participating financially and in the testing during Phase Four. EPRI has pointed to the pilot plant as the only facility in the world capable of turbine testing necessary to permit the projection of actual commercial equipment. Similarly, a number of utilities including Public Service Electric and Gas Company and the Power Authority of the State of New York have promised support during Phase Four testing. Even our Architects and Engineers are so committed to the future of the program that they are willing to forego their fees.

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Thus, it would appear absolutely imprudent to terminate a project that is so close to completion and now of so little cost to the government. Additionally, we should point out that in the original plan the government had anticipated providing 50% of the funding of the demonstration plant. Curtiss-Wright has indicated to the government that it will proceed without such government involvement. Obviously, this will be done in conjunction with a utility which has yet to be firmly identified.

We sincerely believe that the economics, maximum energy utilization potential, and superior environmental performance of this technology dictate the continuation of the project.

We thank you very much for the opportunity to appear before the Committee. We, of course, would be happy to respond to any questions you may have now or at some later date.

PRELIMINARY PFB PILOT PLANT ESTIMATED TERMINATION COSTS

1. VENDOR	$1,357,300
2. REIMBURSEMENT FOR TOTAL ENERGY SYSTEM	5,340,000
3. DISMANTLE FACILITY	2,553,850
4. ENGINEERING ACTIVITY FOLLOWING NOTICE	2,525,000
5. MISCELLANEOUS SEPARATION COST ADMINISTRATION COST RECORDS & STORAGE INDIRECT COST TO BE RECOVERED	2,500,000
SUB TOTAL	$14,276,150
PREPRODUCTION & OTHER COSTS	1,388,000
PROPOSAL, RECRUITMENT, PRE CONTRACT	
TOTAL	$15,664,150

Addendum

During 1981 there was a series of retreats by the government from its earlier PFB plan. The first retreat was from government participation in the commercial scale Demonstration Plant. By that time Curtiss-Wright had expended considerable effort with two utilities and was preparing unsolicited proposals for a Demonstration Plant program keying the start of construction to one year of satisfactory Pilot Plant experience. Early in 1981 DOE made it clear to us that no government participation could be expected, and the proposal efforts were shelved since there was no immediate substitute for the expected government involvement. Curtiss-Wright then proceeded to attempt the organization of a utility consortium as an alternative but, despite favorable FY82 Pilot Plant budgetary activity in the interim, in late September DOE called upon the Company to reduce the requirement for government support during the Operating phase which is to follow completion of construction. By November the company was able to notify DOE that, in addition to Curtiss-Wright, participation by utilities, coal companies and Electric Power Research Institute during the Operating phase appeared likely and that a plan to this effect could be provided to DOE by late February, 1982. In December, DOE notified the Company that it was necessary to reduce still further government support to a level substantially less than that provided by the FY82 Budget. The Company was given until late February to define a program covering not only the Operating phase, but also Construction completion under the stated guideline. The alternative to an acceptable approach is discontinuation of government involvement. Should the contract be cancelled, termination costs are estimated by the Company at $15 million. The Pilot Plant is already 2/3 complete and the termination cost is close to the funding necessary for completion.

As indicated earlier, a total of nearly $50 million of government funds has been expended on this program to date. Approximately $10 million remains in the FY82 Budget. If an early decision is reached to complete the Pilot Plant construction, only $7 to $8 million of new government money would be necessary during FY83 and would result in completion before the end of that fiscal year. An even lower FY83 requirement of $5.5 million could be achieved if DOE accepts the plan recently offered by Curtiss-Wright.

Two important factors have reduced the attraction of the PFB Pilot Plant program to Curtiss-Wright. The first was the cancellation of government plans for a Demonstration Plant. Since, as the government recognized in its original plan, utilities will not invest the very large sum to build a new generating plant unless the technology has been well proven, Curtiss-Wright faces, in the step which must follow the Pilot Plant program, a very large investment of its own in order to make up for the government's absence. The second factor is the requirement by EPRI that its participation during Operation of the Pilot Plant will require the address to new objectives, namely the testing of gas turbine engines built by competitors of Curtiss-Wright. This will permit the generation of a body of technical information with which modifications of engines for PFB commercial use can be reliably defined. While this is clearly a benefit to the utility industry in that a multiplicity of gas turbine manufacturers will be in a position to make early offers of equipment, the competitive advantage of the Pilot Plant program to Curtiss-Wright will be essentially eliminated. In view of the government's intent to disengage from the program, Curtiss-Wright has accepted the concept upon which EPRI is conditioning its own involvement.

The position which DOE has taken to confine its further investment in the PFB Pilot Plant program to that provided by the FY82 Budget is contrary to that of prior Administrations and the present Congress. Further, the delays in making available funding presently authorized is causing not only a delay in the completion of the Pilot Plant program, but also an escalating cost. In effect, DOE's position is placing the decision for continuation of this program in Curtiss-Wright's hands and is doing so by an approach which make the program of ever reducing attractiveness to Curtiss-Wright. Nevertheless, the Company's conviction, now supported by utilities and EPRI, remains that PFB technology is the most attractive advanced coal option realistically attainable and that the Wood-Ridge PFB Pilot Plant is a powerful and necessary tool for settling the key remaining technical issues. This conviction has resulted in the offer by the Company to increase its own participation in the program. Specifically, while no new private sector interests in participation in completion of Construction of the Pilot Plant could be developed by Curtiss-Wright through its efforts of the past several months, the Company has offered to increase its own participation in Construction. In addition, the substantial interest in participation in the Operation of the Pilot Plant by new private sector parties has been developed to the point of almost eliminating any need for government participation in Operation.

Biography - Arnold F. Kossar

Education: Bachelor of Aeronautical Engineering, New York University, 1945
Aeronautical Engineer, Graduate School, Massachusetts Institute of Technology, 1951

Honors: Tau Beta Pi, Sigma Xi, Gamma Alpha Rho

Employment:

1955 - Present	Curtiss-Wright Corporation	
	1966 - Present	Vice President - Engineering
	1955 - 1966	Progressive assignments, especially in the area of gas turbine engines and ground power systems incorporating such engines.
1944 - 1955	Various engineering assignments in aircraft and aircraft engines (except for 1949-51 graduate work)	

Miscellaneous: Member, Industry Advisory Committee, Domestic Policy Review of Impact of Federal Regulation on Innovation, 1978-1979

Member, New Jersey Governor's Science Advisory Panel, 1981-present

Dr. Mechlin.

STATEMENT OF DR. GEORGE F. MECHLIN, VICE PRESIDENT OF RESEARCH AND DEVELOPMENT, WESTINGHOUSE ELECTRIC CORP.

Dr. Mechlin. Mr. Chairman, members of the subcommittee, I appreciate being invited to present comments on energy R. & D. to this subcommittee and my remarks have been submitted for the record.

Consideration of R. & D. for future U.S. energy availability and use is so important to our country that it well deserves your attention in reviewing policies of the present administration, and recommending new perspectives and actions by our Federal Government.

I will begin by summarizing my conclusions on U.S. energy research and development.

First, there is an important leadership role which the Federal Government should play, that is to support selected research and development programs, which improve use and availability of energy for the United States which cannot and will not be supported by private industry acting as individuals in their own prudent and best interests.

Second, many R. & D. projects important to society do not provide adequate corporate profit to justify all R. & D. costs, because benefits tend to accrue to the purchaser/user. Thus, in my judgment, the private sector will not finance many of the energy research, development, and demonstration expenditures formerly funded by the Government.

Third, factors exist which can justify Federal Government funding of energy R. & D., whereas the private sector, given the same opportunity, cannot commit funds. Some of the factors are:

BENEFICIARIES

Government energy R. & D. may benefit all of U.S society and the world compared to a private corporation, which must pay back R. & D. funds by profits from the narrow competitive market captured by its successful R. & D. products.

COST OF MONEY

Government dollars at 10-percent interest rate, for example, would require $4 payback after a 15-year project to return R. & D. investment, while corporate funds at 15 percent would require $8 payback. Payback requirements are further increased by the necessity of successful projects carrying the sunk costs of unsuccessful projects.

CRISIS

Government responsibility to reduce probability of national crisis applies to funding energy R. & D. alternatives for Mid-East oil and its associated balance of payments, just as defense expenditures to prevent military crisis are an accepted reponsibility of the Government.

INTERNATIONAL

U.S. Government antiproliferation and other world stability efforts are aided and abetted by R. & D. which proves U.S. energy leadership to countries with less natural energy resources than the United States.

As the head of research and development for a major electrical manufacturer, I find it impossible to counsel my management to spend more money on R. & D. for which the price mechanism will not recover our incremental costs, and which will not make a competitive return on the capital which must be invested to implement the R. & D. I believe present Government policy which assumes the price mechanism will motivate universally the private sector to implement energy R. & D. formerly funded by our Government should be reevaluated.

Moving from conclusion to history, OPEC demonstrated how the price mechanism will and will not act as a stimulant for energy R. & D. when they increased crude oil prices from $3 to $15 per barrel in 1978, and from $15 to $35 in the period 1978 to 1980. All technologically capable nations, including the United States, initiated and increased R. & D. expenditures to provide new energy options, utilize natural resources, and reduce oil imports.

We are meeting today to discuss whether the OPEC stimulated price increase for oil, and hence all energy, in the resulting climate of economic stress still forces the need for direct Government funding of energy R. & D.; or whether these same forces, plus Government program cancellations, will motivate private industry to take over more energy R. & D. funding responsibility.

Private sector response to OPEC price increases, electric and nonelectric energy use trends, is available for various sectors of the U.S. economy. The residential sector shown in figures 1 and 2 has exhibited a very large deviation from historical trends. Changes were achieved mainly by increased thermal insulation, better heat loss control and more disciplined use of heating, lighting and air conditioning.

The commercial sector, shown in figures 3 and 4, shows a very similar behavior and for the same reasons.

Figures 5 and 6 show the transportation sector's definite trend change response to higher oil prices. Government-edicted more energy efficient automobiles were an important factor. The associated energy R. & D. funding was thus extracted from the public by Government decree. More recently the Department of Transportation reported that motorists traveled 30 billion more miles during the first 9 months of 1981 than in 1980, but used 1.8 billion gallons less gasoline to do it.

Each of these areas shows a large price elasticity, much larger, I think, than the expectations which existed before OPEC forced on us the data. To a planner of industrial R. & D. these data are very discouraging, since they suggest that very large risks exist for R. & D. projects which serve these markets. The risk lies simply in the observed fact that a demonstrated alternative to new technologies namely using less energy—already exists.

Figure 7 shows that only the industrial sector had a history of reducing its nonelectric energy use, on a per-unit basis, prior to the embargo; but did not significantly deviate from its trend line after the embargo, even with higher energy prices. The nonelectric energy use decrease is partly explained by figure 8, which shows an increase in electricity use, and is motivated not only by lower increases in electric energy prices, but also by increased effectiveness of capital, improved labor productivity and more automation. Examples are the heat pump, pollution controls, robots and electric furnaces. The fact that industry did not install cogeneration units which use nonelectric energy, but purchased electricity from electric utilities, which are not included in the industrial sector, also decreased nonelectric and increase electric energy use.

The percentage that actual energy use in 1980 was below the preembargo trend for all end-use sectors is summarized in figure 9, with major trend changs for all except the industrial sector. The total deviation from trend represents 20 times 10 to the 15th Btu equivalent to U.S. transportation energy needs in 1980. As shown in figure 10, all end-use sectors decreased nonelectric energy consumption, except transportation.

In all cases, the use of electricity increased. Increased national acceptance of electricity as the preferred energy form is important, because product objectives of much energy R. & D. have electricity as a common output, for example, fuel cells, photovoltaics, windmills, superconducting generators, bateries, combined cycle plants, fission and fusion reactors.

Preembargo real energy price decrease for electricity, as indicated in figure 11, and postembargo more moderate price increase for electricity provide economic logic for the displacement of nonelectric energy by electric energy in addition to the better energy efficiency available to the user.

With the exception of coal, real energy prices, after discounting for inflation, of all energy forms declined prior to the embargo. After the embargo, heating oil increased 15 percent annually; natural gas, although subject to price controls, increased 13 percent per year; and coal prices increased 9 percent annually.

The price increase for electricity, 4.8 percent per year, was less than for the various fuels used to produce electricity for two reasons:

First, fuel accounts for less than 40 percent of the delivered cost of electricity. In addition, electric utilities shifted from expensive fuel oil toward less expensive coal and uranium as shown in figure 12. For example, electric utility fuel prices in 1980 for oil, coal and uranium were $4.30, $1.35 and 40 cents per million Btu respectively;

Second, electric utilities are a most capital intensive business with large long-term investments required for generation, transmission and distribution. Over 70 percent of the capital equipment required in 1980 was in place prior to 1973, and was thus unaffected by the general inflation that occurred subsequent to the embargo.

As a nation, we are indeed fortunate to have had available the reserves of generation equipment bought in the past and at prices which are a mere fraction of future replacement costs. Within this decade we will exhaust this source of additional availability of electricity. What are the future prospects?

The economic environment in electric utilities does not favor major capital investment in new energy efficient equipment. Utility financing alternatives for purchase of major new capital equipment are: High interest rate loans, downgraded bonds saleable only at high discounts, or diluted stockholder equity by sale of common stock at a market price 60 to 80 percent of book value. These conditions do not favor our recovery of R. & D. expenditures by selling the needed improvement in energy conversion equipment to our major customers, the electric utility industry.

In addition, efficiency improvements of energy conversion equipment result in lower fuel bills for the utilities and their customers—the residential, commercial and industrial users—who benefit because of fuel pass-through clauses but not necessarily the manufacturer who improves the machines.

Under these conditions no prudent manufacturer will be attracted to R. & D. in the electrical energy area unless a significant portion of the cost is shared by the ultimate beneficiary, the public, through Government support of R. & D.

Returning to the history of nonelectrical industrial energy use, the fact that the trend, pre- and post-OPEC, did not change seems best explained by the observation that capital formation is the factor which limits progress. Simply put, industry has been putting all the capital it can afford into improving energy efficiency. While incentives for investment may have increased, post-OPEC, inflation apparently has made the capital formation correspondingly difficult so that the rate of real progress in energy productivity remained constant. To the planner of industrially funded R. & D. this also is a discouraging factor. Again, the risk of project success must be judged high, since the prospective industrial customers have demonstrated limited capital resources. If real progress from R. & D. effort is to be made in this area the risk must be shared by the Government on behalf of the people who are the ultimate beneficiaries.

Let me give you a specific example of how industry evaluates R. & D. projects. Figure 13 illustrates the life cycle economics of developing a superconducting generator. Basic research on superconducting material started at Westinghouse in 1948, with the first practical superconductor, niobium titanium, produced in the 1960's. A 5-megawatt superconducting generator was funded by Westinghouse in 1971 and a 300-megawatt superconducting generator, cost shared with EPRI, is scheduled on line in 1986.

If forecasts of success, costs, schedules and utility market acceptance are correct, capitalized benefits to U.S. utilities by the year 2000 are forecast at $1.3 billion—or $200 million, present worthed at a 15-percent discount—plus energy savings equivalent to 10 million barrels of oil. The same assumptions indicate Westinghouse cash flow, discounted at 15 percent, will break even in 1995, and be $35 million positive by the year 2000, which is one-sixth the benefit enjoyed by the utilities and, hence, by the public.

I would like to cite three important examples of Government funded energy R. & D. sponsored by your committee which have made excellent progress and demonstrate promise of significant benefit to the United States, but are not yet economic enough to warrant complete funding by the private sector.

First, the long-term promise of solar energy justifies Government support to provide renewable energy benefits to the United States. With Government support, we have developed the dendritic web type of photovoltaic cell, which offers very economic use of silicon crystals and a conversion efficiency from solar installation to electricity of ~12 percent. We are now delivering production line 60-watt modules to two west coast utilities. R. & D. is still a vital necessity to reduce costs for future units because present costs of ~$10 per peak watt are not economic for most applications.

Second, coal gasification at Westinghouse was motivated by a desire to convert coal to a gas fuel compatible with combustion turbines, and thus reduce oil imports for generating electricity.

An R. & D. project started with 70-percent funding from the Office of Coal Research resulted in an 18 ton-per-day process development unit which has operated successfully for over 5 years on many types of coal and with production of low and medium Btu gas. Devolitization and gasification, originally requiring two major vessels, are accomplished now in a single vessel. Lack of R. & D. funding for a scaled up demonstration unit to convince a domestic customer, has intensified our international negotiations with a prospective partner who has recognized our gasifier as probably 10 percent lower in capital investment, 15 percent better in energy efficiency and environmentally more benign than other gasifiers operating today.

Third, R. & D. fuel on cells, supported by Government and Westinghouse funds, has resulted in substantial improvements in operating performance for the phosphoric acid fuel cell at 375° F, and in high efficiency for the high-temperature solid oxide fuel cell at 1,800° F. The high-temperature fuel cell which does not require any expensive materials of construction, and with an electric efficiency above 50 percent, has attracted favorable interest for utility, industrial and Government application.

Our fiscal year 1983 budget recommendations for the programs I have just mentioned—photovoltaic, coal gasification and fuel cell—are compared to fiscal year 1982 Government budgets and administration proposed budgets for fiscal year 1983 in table 1.

As an indication of cost sharing by the private sector for the Westinghouse coal gasification concept, funding for this program from 1972 to 1981 was $55.6 million and $33.8 million, from the Government and private sector respectively. When 1982 expenditures are included the source of funds will be $66 million and $63 million, from the Government and private sector respectively.

Turning from Government funded to private sector funded energy R. & D., electric utilities are funding short-term energy R. & D. to upgrade present equipment and improve reliability. As motivation, figure 14 illustrates how the cost to a utility of 1 day of unavailability for a fossil fueled 500-megawatt powerplant has increased from about $30,000 in 1970 to over $500,000 in 1980. Longer range R. & D. is not active because of lack of funds and growth in the electric utility industry today.

Just because of the temporary world surplus of oil and gas, and the fact that OPEC has not doubled oil prices lately, does not mean that our Government should completely abandon energy R. & D. and synfuels to the expedient of price mechanism motivation. I am pleased that Congress is evaluating Government policies on R. & D. as indicated by:

The hearings today, the hearings before Chairman Marilyn Bouquard's Subcommittee on Energy Research and Production; and the increase and balanced funding for fossil, conservation, and nuclear programs proposed by the Senate Energy Committee; and, once again, these hearings.

I hope you will convince the administration that certain energy R. & D. programs can provide significant benefits to the people of the United States, which will be available only if the programs are funded by our Government.

Thank you.

Mr. **Fuqua**. Thank you very much, Dr. Mechlin.

[The prepared statement of Dr. Mechlin follows:]

R&D for Future U. S. Energy

Dr. George F. Mechlin
Vice President - Research and Development
Westinghouse Electric Corporation

March 23, 1982

Subcommittee on Energy Development and Application
of the
Committee on Science and Technology

U. S. Congress
Washington, D.C.

R&D FOR FUTURE ENERGY

Mr. Chairman, Members of the Subcommittee on Energy Development and Application, staff, and fellow speakers, good morning. I am George Mechlin, Vice President for Research and Development of Westinghouse Electric Corporation. I appreciate being invited to present comments on energy R&D to this Subcommittee, of the Committee on Science and Technology of the U. S. Congress.

Consideration of R&D for future U. S. energy availability and use is so important to our country, that it well deserves your attention in reviewing policies of the present administration, and recommending new perspectives and actions by our federal government.

I will begin by summarizing my conclusions on U. S. energy research and development:

1. There is an important leadership role which the Federal Government should play, i.e., support selected research and development programs, which improve use and availability of energy for the U. S., but which cannot and will not be supported by private industry acting as individuals in their own prudent and best interests.

2. Many R&D projects, important to society, do not provide adequate corporate profit to justify all R&D costs, because benefits tend to accrue to the purchaser-user. Thus, in my judgement, the private sector will not finance many of the energy research, development and demonstration expenditures formerly funded by the government.

3. Factors exist which can justify Federal Government funding of energy R&D, whereas the private sector, given the same opportunity, cannot commit funds. Some of these factors are:

- o Beneficiaries -- Government energy R&D may benefit all of U. S. society, and the world, compared to a private corporation, which must payback R&D funds by profits from the narrow competitive market captured by its successful R&D products.

- Cost of Money -- Government dollars at 10% interest rate, for example, would require four dollars payback after a 15 year project to return R&D investment; while corporate funds at 15% would require eight dollars payback. Payback requirements are further increased by the necessity of successful projects carrying the sunk costs of unsuccessful projects.
- Crisis -- Government responsibility to reduce probability of national crisis, applies to funding energy R&D alternatives for Mid-East oil and its associated balance of payments crisis, just as defense expenditures to prevent military crisis are an accepted responsibility of government.
- International -- U. S. Government antiproliferation, and other world stability efforts, are aided and abetted by R&D which proves U. S. energy leadership to countries with less natural energy resources than the U. S.

4. As head of research and development for a major electrical manufacturer, I find it impossible to counsel my management to spend more money on R&D for which the price mechanism will not recover our incremental costs, and which will not make a competitive return on the capital which must be invested to implement the R&D. I believe present government policy which assumes the price mechanism will motivate universally the private sector to implement energy R&D, formerly funded by our government, should be reevaluated.

Moving from conclusions to history, OPEC demonstrated how the price mechanism will and will not act as a stimulant for energy R&D, when they increased crude oil prices from $3 to $15 per barrel in 1978, and from $15 to $35 in the period 1978-80. All technologically capable nations, including the U. S., initiated and increased R&D expenditures to provide new energy options, utilize natural resources, and reduce oil imports.

We are meeting today to discuss whether the OPEC stimulated price increase for oil, and hence all energy, in the resulting climate of economic stress, still forces the need for direct government funding of energy R&D; or whether these same forces, plus government program cancellations, will motivate private industry to take over more energy R&D funding responsibility.

Private sector response to OPEC price increases, electric and nonelectric energy use trends, is available for various sectors of the U. S. economy. The residential sector shown in Figures 1 and 2, has exhibited a very large deviation from historical trends. Changes were achieved mainly by increased thermal insulation, better heat loss control, and more disciplined use of heating, lighting and air conditioning.

The commercial sector, shown in Figures 3 and 4, shows a very similar behavior and for the same reasons.

Figures 5 and 6 show the transportation sector's definite trend change response to higher oil prices. Government-edicted more energy efficient automobiles were an important factor. The associated energy R&D funding was thus extracted from the public by government decree. More recently the Department of Transportation reported that motorists traveled 30 billion more miles during the first nine months of 1981 than in 1980, but used 1.8 billion gallons less gasoline to do it.

Each of these areas shows a large price elasticity - much larger I think - than the expectations which existed before OPEC forced on us the data. To a planner of industrial R&D, these data are very discouraging, since they suggest that very large risks exist for R&D projects which serve these markets. The risk lies simply in the observed fact that a demonstrated alternative to new technologies - namely, using less energy - already exists.

Figure 7 shows that only the industrial sector had a history of reducing its nonelectric energy use, on a per-unit basis, prior to the embargo; but did not significantly deviate from its trend line after the embargo, even with higher energy prices. The nonelectric energy use decrease is partly explained by Figure 8, which shows an increase in electric energy is motivated, not only by lower increases in electric energy prices, but also by increased effectiveness of capital, improved labor productivity, and more automation. Examples are the heat pump, pollution controls, robots, and electric furnaces. The fact that industry did not install cogeneration units, which use nonelectric energy, but purchased electricity from electric utilities, which are not included in the industrial sector, also decreased nonelectric and increased electric energy use.

The percentage that actual energy use in 1980 was below the pre-embargo trend for all end-use sectors, is summarized in Figure 9, with major trend changes for all except the industrial sector. The total deviation from trend represents about 20 10^{15} BTU-equivalent to U. S. transportation energy needs in 1980. As shown in Figure 10, all end-use sectors decreased nonelectric energy consumption, except transportation.

In all cases the use of electricity increased. Increased national acceptance of electricity as the preferred energy form is important, because product objectives of much energy R&D have electricity as a common output, i.e., fuel cells, photovoltaics, wind mills, superconducting generators, batteries, combined cycle plants, fission reactors, and fusion.

Pre-embargo real energy price decrease for electricity, as indicated in Figure 11, and post embargo more moderate price increase for electricity, provide economic logic for the displacement of nonelectric energy by electric energy in addition to the better energy efficiency available to the user.

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- First, fuel accounts for less than 40% of the delivered cost of electricity. In addition electric utilities shifted from expensive fuel oil, toward less expensive coal and uranium as shown in Figure 12. For example, electric utility fuel prices in 1980 for oil, coal and uranium were $4.30, $1.35, and $.40 per million BTU respectively.
- Second, electric utilities are a most capital intensive business, with large, long term investments, required for generation, transmission and distribution. Over 70% of the capital equipment required in 1980 was in place prior to 1973, and was thus unaffected by the general inflation that occurred subsequent to the embargo.

As a nation, we are indeed fortunate to have had available the reserves of generation equipment bought in the past and, at prices which are a mere fraction of future replacement costs. Within this decade, we will exhaust this source of additional availability of electricity. What are the future prospects?

The economic environment in electric utilities does not favor major capital investment in new energy efficient equipment. Utility financing alternatives for purchase of major new capital equipment are: high interest rate loans, downgraded bonds saleable only at high discounts, or diluted stockholder equity by sale of common stock at a market price 60-80% of book value. These conditions do not favor our recovery of R&D expenditures by selling the needed improved energy conversion equipment to our major customer, the electric utility industry.

In addition, efficiency improvements of energy conversion equipment, result in lower fuel bills for the utilities, and their customers, the residential, commercial, and industrial users, who benefit, because of fuel pass-through clauses, but not necessarily the manufacturer who improves the machines.

Under these conditions no prudent manufacturer will be attracted to R&D in the electrical energy area unless a significant portion of the cost is shared by the ultimate beneficiary, the public, through government support of R&D.

Returning to the history of non-electrical industrial energy use, the fact that the trend, pre and post OPEC, did not change, seems best explained by the observation that capital formation is the factor which limits progress. Simply put, industry has been putting all the capital it can afford into improving energy efficiency. While incentives for investment may have increased, post OPEC, inflation apparently has made capital formation correspondingly difficult, so that the rate of real progress in energy productivity remained constant. To the planner of industrially funded R&D, this also is a discouraging factor. Again, the risk of project success must be judged high, since the prospective industrial customers have demonstrated limited capital resources. If real progress from R&D effort is to be made in this area, the risk must be shared by the government on behalf of the people who are the ultimate beneficiaries.

Let me give you a specific example of how industry evaluates R&D projects. Figure 13 illustrates the life cycle economics of developing a superconducting generator. Basic research on superconducting material started at Westinghouse in 1948, with the first practical superconductor, niobium titanium produced in the 1960's. A 5 MW superconducting generator (SCG) was funded by Westinghouse in 1971, and a 300 MW SCG, cost shared with EPRI, is scheduled on line in 1986.

If forecasts of success, costs, schedules and utility market acceptance are correct, capitalized benefits to U. S. utilities by the year 2000 are forecast at $1.3 billion ($200 millions, present worthed at 15%) plus energy savings equivalent to 10 million barrels of oil. The same assumptions indicate Westinghouse cash flow, discounted at 15%, will break even in 1995, and be $35 million positive by the year 2000, which is one sixth the benefit enjoyed by the utilities.

I'd like to cite three important examples of government funded energy R&D sponsored by your committee, which, have made excellent progress, and demonstrate promise of significant benefit to the U. S., but are not yet economic enough to warrant complete funding by the private sector.

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An R&D project started, with 70% funding from OCR, resulted in an 18 ton per day process development unit (PDU), which has operated successfully for over five years, on many types of coal, and with production of low and medium-BTU gas. Devolitization and gasification, originally requiring two major vessels, are accomplished now in a single vessel. Lack of R&D funding for a scaled up demonstration unit to convince a domestic customer, has intensified our international negotiations, with a prospective partner who has recognized our gasifier as probably 10% lower in capital investment, 15% better in energy efficiency, and environmentally more benign than other gasifiers operating today.

Third, R&D on fuel cells, supported by government and Westinghouse funds, has resulted in substantial improvements in operating performance for the phosphoric acid fuel cell at 375^{o}F, and in high efficiency for the high temperature solid oxide fuel cell at

1800°F. The high temperature fuel cell which does not require any expensive materials of construction, and with an electric efficiency above 50%, has attracted favorable interest for utility, industrial, and government application.

Our FY 1983 budget recommendations for the programs I have just mentioned i.e., photovoltaic, coal gasification, and fuel cell, are compared to FY 1982 government budgets and administration proposed budgets for FY 1983, in Table 1.

TABLE 1
R&D BUDGETS

	ADMINISTRATION FY 1982	ADMINISTRATION FY 1983	RECOMMENDATION FY 1983
Photovoltaic	74	27	45
Coal Gasification	53	10.5	20
Fuel Cell	35	9.5	35

As an indication of cost sharing by the private sector for the Westinghouse coal gasification concept, funding for this program, from 1972 - 1981, was $55.6 and $33.8 million, from the government and private sector respectively. When 1982 expenditures are included, source of funds will be $66 and $63 million, from the government and private sector respectively.

Turning from government funded to private sector funded energy R&D, electric utilities are funding short term energy R&D, to upgrade present equipment, and improve reliability. As motivation, figure 14 illustrates how the cost to a utility, of one day of unavailability for a fossil fueled 500 MW power plant, has increased from about $30,000 in 1970 to over $500,000 in 1980. Longer range R&D is not active, because of lack of funds and growth in the electric utility industry today.

Just because of the temporary world surplus of oil and gas, and the fact that OPEC has not doubled oil prices lately, does not mean that our government should completely abandon energy R&D and synfuels to the expedient of price mechanism motivation. I am pleased that congress is evaluating government policies on R&D as indicated by:

- o hearings before Chairman, Marilyn Bouquard's Subcommittee on Energy Research and Production, which included testimony on how billions of U.S. dollars might be saved by the results of R&D on higher voltage transmission of electricity, and amorphous metals for transformers.
- o the increased, and balanced funding for fossil, conservation, and nuclear programs proposed by the Senate Energy Committee
- o and these hearings today.

I hope you will convince the administration that certain energy R&D programs can provide significant benefits to the people of the United States, which will be available only if the programs are funded by our government. Thank you.

RESIDENTIAL ENERGY USE PER HOUSEHOLD

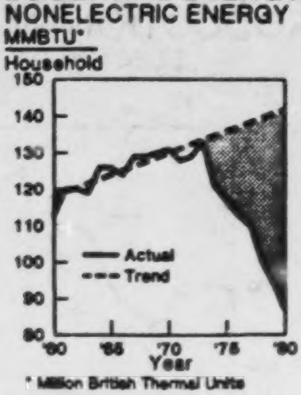


Figure 1

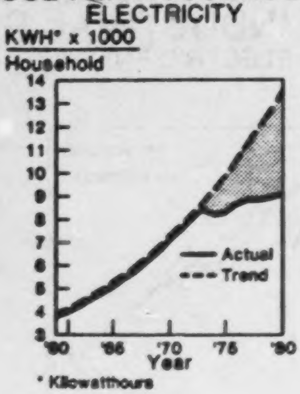


Figure 2

COMMERCIAL ENERGY USE PER COMMERCIAL EMPLOYEE

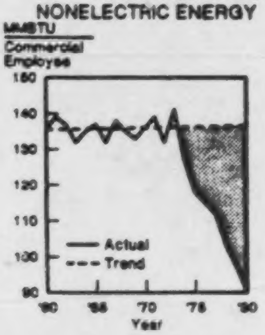


Figure 3

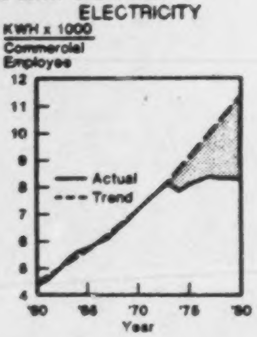


Figure 4

TRANSPORTATION ENERGY USE PER VEHICLE

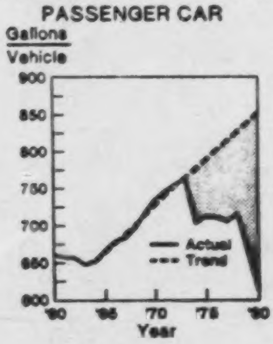


Figure 5

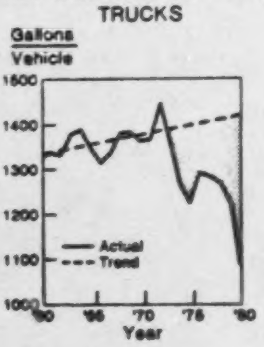
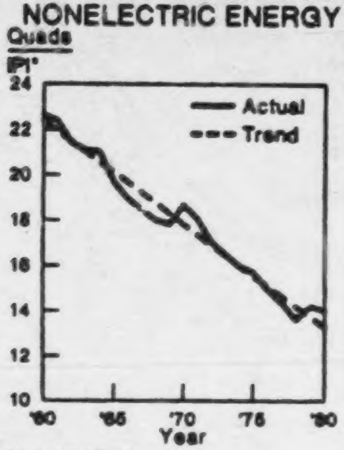


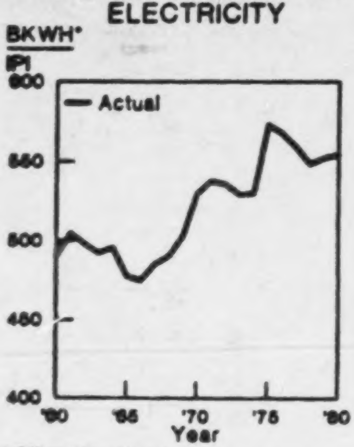
Figure 6

INDUSTRIAL ENERGY USE PER INDUSTRIAL PRODUCTION INDEX



* Industrial Production Index

Figure 7



* Billion Kilowatthours

Figure 8

PERCENTAGE THAT ACTUAL ENERGY USE IN 1980 WAS BELOW THE PRE-EMBARGO TREND

End-User Sector	Nonelectric Energy	Electricity	Total
Residential	-39	-33	-35
Commercial	-34	-27	-31
Industrial	N.C.	N.C.	N.C.
Transportation	-22	--	-22
TOTAL ECONOMY	-20	-20	-20

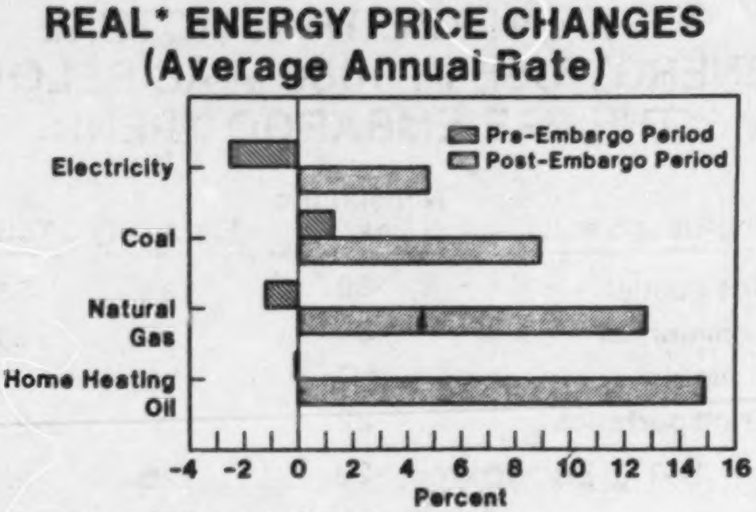
N.C. = No Change Discernable

Figure 9

CHANGES IN ENERGY CONSUMPTION 1973 TO 1980 (PERCENT)

End-Use Sector	Nonelectric Energy	Electricity
Residential	-17.6	+23.8
Commercial	-15.2	+25.7
Industrial	- 3.4	+18.8
Transportation	+ 0.6	--
TOTAL ECONOMY	- 5.6	+22.2

Figure 10



*Increases in Excess of General Inflation

Figure 11

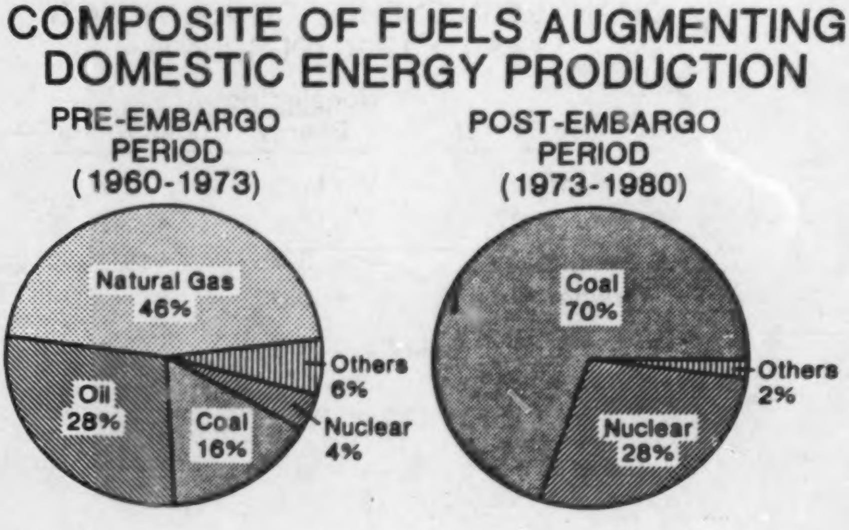


Figure 12

SUPERCONDUCTING GENERATOR

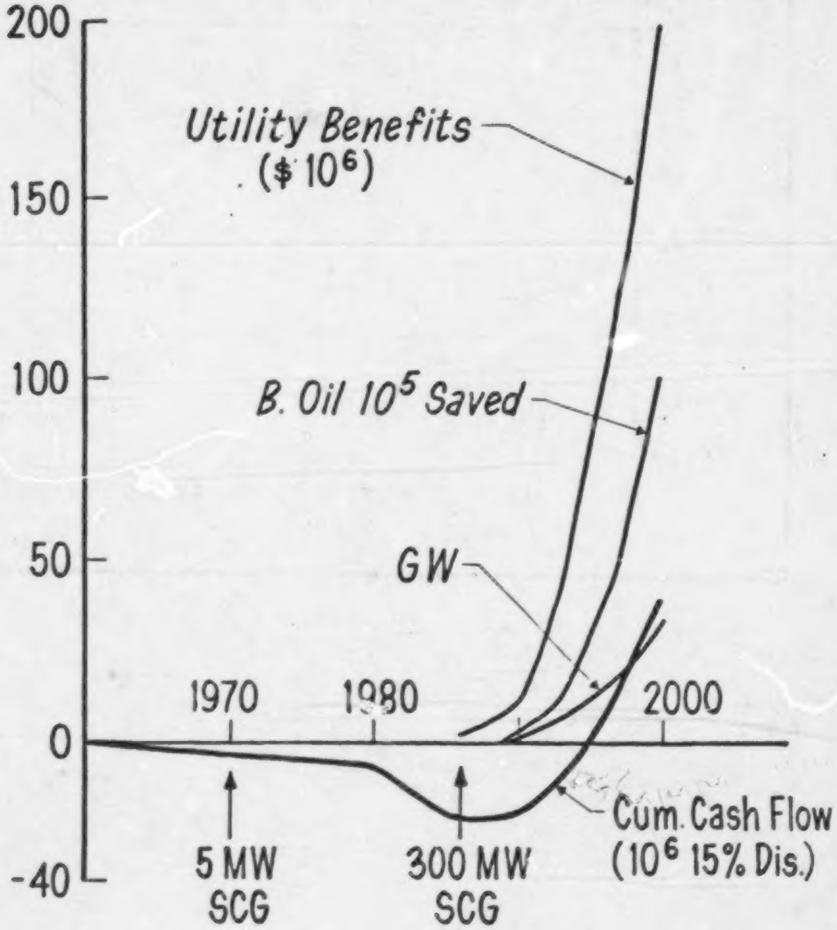


Figure 13

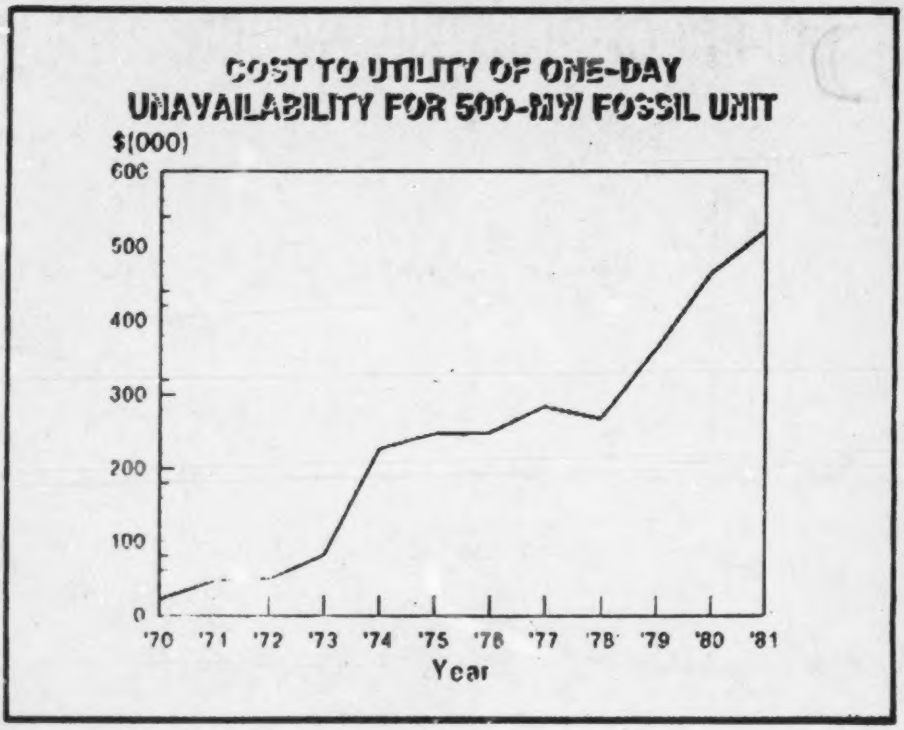

COST TO UTILITY OF ONE-DAY
UNAVAILABILITY FOR 500-MW FOSSIL UNIT
$(000)
600
500
400
300
200
100
0
'70
'71
'72
'73
'74
'75
'76
'77
'78
'79
'80
'81
Year

Figure 14

Mr. FUQUA. Mr. Ross, we are happy to have you here today.

STATEMENT OF ROBERT R. ROSS, DIRECTOR OF ENGINEERING, SEMINOLE ELECTRIC COOPERATIVE, INC., ON BEHALF OF THE FUEL CELL USERS GROUP; ACCOMPANIED BY GARY TIPPS AND RICHARD BREITMOSER

Mr. ROSS. Thank you, Mr. Chairman.

I have previously submitted a written statement which I understand will be included in its entirety in the record.

Mr. FUQUA. Yes; that is correct.

Mr. ROSS. In the interest of time I will be brief and will try to summarize the major points that are contained within that written statement.

I appear here today on behalf of the Fuel Cell Users Group as well as Seminole Electric Cooperative in support of a strong and progressive research program on fuel cells. Appearing with me in support of the program are Gary Tipps of the Tampa Electric Co. and Richard Breitmoser of the Jacksonville Electric Authority.

As you will note from this representation, and the list of FCG participants which are included as an attachment to my prepared statement, the fuel cell program has a very broad base of support not only within the State of Florida but throughout the Nation. The 55 member utilities of the Fuel Cell Users Group represent 40 percent of the electric generating capacity in the United States. These members are working together with the Electric Power Research Institute, the manufacturers and the Department of Energy to identify and resolve the issues which are obstacles to the commercialization of the fuel cell.

In the prepared statement I try to make three significant points:

The first point is that the fuel cell presents certain unique advantages which can contribute to the solution of environmental and technical problems encountered in siting, constructing, and operating electrical power supply facilities.

The second point is the Fuel Cell Users Group offers its full support in a partnership role in joint efforts toward rapid commercialization of the phosphoric acid fuel cell for electric power generation.

The third point, and primary reason we are here today, is to urge Federal funding for continued technological research and development for the fuel cell commensurate with its potential for early, successful commercialization. We respectfully recommend and urge that fiscal year 1983 funding be continued at the fiscal year 1982 level of $34.5 million, and that Federal priorities be established to provide support for commercialization of the technology.

I believe the advantages of the fuel cell are well known to this committee and in the interest of time I won't go into the details of those, but we would like to note the many environmental advantages, the advantages of the modular construction, the short lead times and other things which are very important to the utility industry with the present state of affairs.

I would like to concentrate a little bit on the Users Group in a partnership role. The electric utility industry has made a major commitment to the development and commercialization of fuel cell powerplants. In the decade from 1971 to 1981 EPRI, the electric utilities, fuel cell manufacturers, and others invested over $150 million in the development of the phosphoric acid fuel cell. You might compare this to Federal funds during that time which were approximately one-third of that amount.

The Users Group and individual utility members are prepared to continue to provide support in areas which will lead toward the goal of commercial introduction of the fuel cells as a viable utility powerplant. We are prepared to work with the manufacturers to provide guidance in development of the final specifications for the fuel cell, to assist in identifying early specifications for the fuel cell, to assist in identifying early users; to collect data, analyze costs, and assess the availability of the technology; to identify gaps in research which are either not currently being funded or are inadequately funded; and to support early purchasers by providing them with evaluations of the proposed commercial configuration.

Our major concern is that present financial problems facing the utility industry will not allow research and development to proceed without continued financial assistance from the Federal Government. The industry is not in a position to assume the financial risks associated with the high cost of technical research and experimentation needed to refine and prove the technology.

Without an ongoing technology development program, the utility industry will not be assured that a final acceptable commercial product will be developed. Consistency and stability in Government support is essential for the successful early commercialization of the fuel cell technology.

In addition, we should not overlook the interest shown by foreign countries in the fuel cell technology. Tokyo Electric is installing and will soon demonstrate a 4.8-megawatt powerplant similar to the Con Ed plant, but with an advanced stack design. Unless we move expeditiously to commercialize the technology in this country we could find ourselves purchasing fuel cell powerplants from Japan where, historically, the Government provides strong financial support for research and commercial development of new technology.

In conclusion, the members of the Fuel Cell Users Group find that the promise of the fuel cell is extraordinary and we are committed to our continuing efforts in the commercialization program. We believe that a continuation of the industry and Government partnership in the research and development of fuel cell technology would be consistent with our national priorities of energy self-sufficiency, economic well being, and environmental health. We therefore urge that the fuel cell funding be continued at the fiscal year 1982 level of $34.5 million for the next year.

Thank you for this opportunity to present this statement on behalf of the Fuel Cell Users Group of the electric utility industry.

[The prepared statement of Mr. Ross follows:]

STATEMENT ON THE FY 1983 FUEL CELL BUDGET
OF THE DEPARTMENT OF ENERGY
presented to
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
U. S. HOUSE OF REPRESENTATIVES
BY
ROBERT R. ROSS, DIRECTOR OF ENGINEERING
SEMINOLE ELECTRIC COOPERATIVE, INC.
on behalf of
THE FUEL CELL USERS GROUP
March 23, 1982

MR. CHAIRMAN AND MEMBERS OF THE COMMITTEE, my name is Robert R. Ross. I am a member of the Management Committee of the Fuel Cell Users Group of the electric utility industry and Director of Engineering for Seminole Electric Cooperative, Inc. I appear before you today on behalf of the Fuel Cell Users Group as well as Seminole Electric Cooperative, Inc. in support of a strong and progressive research program on fuel cells. Appearing with me in support of the program are Gary Tipps of Tampa Electric Company and Rick Breitmoser of Jacksonville Electric Authority.

The Fuel Cell Users Group is broadly based with participation by cooperatives, public power agencies and investor-owned utilities. The 55 member utilities represent 40 percent of the electric generating capacity in the United States. These utilities are all potential users of the fuel cell technology and all see great potential for the fuel cell to fulfill a variety of unique utility needs. The member utilities are working together with the Electric Power Research Institute (EPRI), the manufacturers and the Department of Energy to identify and resolve the issues which are obstacles to the commercialization of the fuel cell.

In this statement we would like to make three significant points:

1. The fuel cell presents certain unique advantages which can contribute to the solution of environmental and

technical problems encountered in siting, constructing and operating electrical power supply facilities.

2. The Fuel Cell Users Group offers its full support in a partnership role in joint efforts toward rapid commercialization of the phosphoric acid fuel cell for electric power generation.
3. We urge Federal funding for a continued technological research and development for the fuel cell commensurate with its potential for early, successful commercialization. We respectfully recommend and urge that FY1983 funding be continued at the FY1982 level ($34.5 million) and that Federal priorities be established to provide support for commercialization of the technology.

FUEL CELL ADVANTAGES

We are firmly convinced that the commercialization of the fuel cell is in the national interest because of its environmental, economic and efficiency attributes. Conceivably, it could revolutionize the energy industry in the way solid-state components have changed the electronic industry.

The fuel cell, because of its minimal land requirements, modular construction and low environmental impact, will be relatively easy to site. A utility scale facility would occupy less than one acre of land. Factory-manufactured modules would be delivered by truck and assembled in periods of months. The fuel cell requires virtually no water supply. Its operation is relatively quiet and emissions such as oxides of nitrogen and sulfur are virtually non-existent. In many cases a fuel cell could be installed without constructing additional transmission lines. The modular construction and prefabrication of major components is expected to permit the installation and start-up operation of a fuel cell on a two-year time scale. This is particularly important to utilities in terms of planning flexibility, local siting and also in delaying capital investment until additional capacity is required.

Generation efficiencies of 41 percent to 50 percent can be expected from the fuel cell. (In contrast to 28 percent to 38 percent for conventional thermal power plants.) This fuel saving or conservation quality is seen to be very important in today's energy climate. The fuel cell also operates efficiently at partial loads which allows the utility much flexibility in utilizing the facility for intermediate or peaking service. Additional efficiencies can be realized by utilizing the fuel cell in a cogeneration mode. By fitting the fuel cell with equipment to recover rejected heat, the power plants can supply steam or hot water for process industries and for building heating systems. Overall fuel conversion efficiencies of 80 percent are possible utilizing fuel cells in this manner.

Fuel flexibility is also expected to be an advantage. First, generation fuel cells will be able to use petroleum or coal-derived distillates, natural gas, methane, synthetic gas or fuel grade methanol. These fuels include methane-rich gases which might be produced from land fills, sewage, feedlot wastes and other non-conventional sources. The relatively small scale of fuel cell power plants enables the utility to take advantage of localized fuel availabilities.

THE USERS GROUP IN A PARTNERSHIP ROLE

The electric utility industry has already made a major commitment to the development and commercialization of fuel cell power plants. In the decade from 1971 to 1981, EPRI, electric utilities, fuel cell manufacturers and others invested over $150 million in the development of the phosphoric acid fuel cell, compared to Federal funds of approximately one-third that amount. This does not include substantial in-kind contributions by members of the Fuel Cell Users Group in their support of commercialization efforts.

The Users Group and individual utility members are prepared to continue to provide support in areas which will lead toward the goal of commercial introduction of the fuel cell as a viable utility power plant. We are prepared to work with the manufacturers to provide guidance in development of the final specifications for the fuel cell, to assist in identifying early specifications for the fuel cell, to assist in identifying early users; to collect data, analyze costs and assess the availability of the technology; to identify gaps in research which are either not currently being funded or are inadequately funded; and to support early purchasers by providing them with evaluations of the proposed commercial configuration.

Our major concern is that present financial problems facing the utility industry will not allow research and development to proceed without continued financial assistance from the Federal government. The industry is not in a position to assume the financial risks associated with the high cost of technical research and experimentation needed to refine and prove the technology.

Without an ongoing technology development program, the utility industry will not be assured that a final acceptable commercial product will be developed. It is here that we propose that the Federal government could assume its proper role by continued financial support of technology programs aimed at upgrading fuel cell performance in critical areas, especially those which have been identified by the utilities themselves.

Consistency and stability in government support is essential for the successful early commercialization of the fuel cell technology. Utilities are not likely to make the major commitments required for the purchase of the early units unless there is assurance of Federal government support and determination to see the project through to a successful completion.

In addition, we should not overlook the interest shown by foreign countries in the fuel cell technology. Tokyo Electric is installing and will soon demonstrate a 4.8 MW power plant similar to the Con Ed plant, but with an advanced stack design. Unless we move expeditiously to commercialize the technology in this country, we could find ourselves purchasing fuel cell power plants from Japan where, historically, the government provides strong financial support for research and commercial development of new technology.

CONCLUSION

In conclusion, the members of the Fuel Cell Users Group find that the promise of the fuel cell is extraordinary and we are committed to our continuing efforts in the commercialization program. We believe that a continuation of the industry and government partnership in the research and development of fuel cell technology would be consistent with our natural priorities of energy self-sufficiency, economic well being and environmental health. We therefore urge that the fuel cell funding be continued at the FY1982 level ($34.5 million) for next year.

Thank you for the opportunity to present this statement on behalf of the Fuel Cell Users Group of the electric utility industry.

ATTACHMENTS:

1. List of Fuel Cell Users Group Members
2. January 15 letter to The President from William H. Corkran, Jr., Acting Chairman, Fuel Cell Users Group
3. Resolution on "Federal Support for Fuel Cell Commercialization" confirmed by members of the National Rural Electric Cooperative Association at the annual meeting in Atlanta, February 10, 1982, attended by more than 9,700 people.

FUEL CELL USERS GROUP MEMBERS

Adams Electric Cooperative
Allegheny Electric Cooperative
Alpena Power Company
American Public Power Association
Anchorage Municipal Light & Power
Arizona Public Service Company
Baltimore Gas & Electric Company
Basin Electric Cooperative
Boston Edison Company
Brazos Electric Power Cooperative
Buckeye Power, Inc.
Central Hudson Gas & Electric Corporation
Central Illinois Light Company
Colorado Ute Electric Assn., Inc.
Commonwealth Edison Company
Consolidated Edison Co. of NY
The Dayton Power & Light Company
Delmarva Power & Light Company
Easton Utilities Commission
Edison Electric Institute
Electric Power Research Institute
Green Mountain Power Corporation
Hoosier Energy Division, REC, Inc.
Hydro-Quebec
Idaho Power Company
Jacksonville Electric Authority
Kansas City Power & Light Company
Lee County Electric Cooperative
Lincoln Electric System
Long Island Lighting Company
Massachusetts Municipal Wholesale Electric Company
Memphis Light, Gas & Water
Mississippi Power & Light Company
Missouri Basin Municipal Power Agency
National Rural Electric Cooperative Association
Niagara Mohawk Power Corporation
Northeast Utilities Service Company
Ohio Edison Company
Ontario Hydro
Philadelphia Electric Company
Provo City Power
Public Service Company of Oklahoma
Public Service Electric & Gas Co.
Rural Electrification Administration
San Diego Gas & Electric Company
Santa Clara Electric Department
Seminole Electric Cooperative, Inc.
Southern California Edison Company
Southern Company Services, Inc.
Southern Maryland Electric Coop.
Tacoma Dept. of Public Utilities
Tampa Electric Company
Taunton Municipal Lighting Plant
Tennessee Valley Authority
Tokyo Electric Company
Toledo Edison Company
United Power Association
Virginia Electric Power Company

Associate Members

Biltmore Development Company
Burns & McDonnell Engineering Co.
Burns & Roe, Inc.
Ebasco Business Consulting Services, Inc.
Johnson Matthey Research Centre
NUS Corporation

January 15, 1982

The President
The White House
Washington, D.C. 20500

Dear Mr. President:

I am writing to encourage continuation of adequate funding for research and development of fuel cell technology in the Department of Energy for FY 1983 and beyond. It has been reported that funding of $9.5 million for this technology is being considered for FY 1983, in contrast to appropriations of $34.5 million for FY 1982. This reduced funding level would undercut the efforts of the utility industry to commercialize fuel cell power plants.

The electric utility industry in the United States has made a major commitment to development and commercialization of fuel cell power plants. Between 1971 and 1981, the electric-utility sector (utility companies, fuel cell manufacturers and others) spent over $150 million on the phosphoric acid fuel cell program, as compared to federal funds of approximately one-third this amount. In 1980, the Fuel Cell Use s Group of the Electric Utility Industry, Inc. was established to promote commercialization of fuel cell technology. The Users Group now has 64 public, private, and rural cooperative members representing some 40 percent of electric power sales in the United States. The Users Group, individual utilities, and the fuel cell manufacturers are in the process of developing an industry-supported program for fuel cell commercialization.

The industry commitment reflected in these actions is evidence of our conviction that the fuel cell offers important technical, financial, and environmental benefits to electric utilities. To realize these benefits, it is vital, as pointed out in your recent statement on nuclear policy, ". . . that the utilities - investor-owned, public, and coops - be able to develop new generating capacity to permit them to supply their customers at the lowest cost, be it coal, hydro, or new technologies such as the fuel cell." Moreover, the commitment of our industry to fuel cell commercialization is squarely in accord with your policy of relying on industry for the deployment of new energy technology, while limiting federal funding to higher risk research and development activities.

Although industry is prepared to take the lead in fuel cell commercialization, we can attain our goal only if industry and government work in partnership, each playing its proper role in an integrated and stable program. The need for such a partnership is particularly acute in developing new technologies in the electric utility industry. In a report to Congress of September 28, 1981, dealing with the ability of our industry to adopt new technologies, the General Accounting Office noted that ". . . the risk [of commercialization] is not just limited to development of new technology. Huge financial risk is associated with incorporating a newly developed and working technology into an established and complex electric power system so that it is both economical and reliable."

Because most electric rates are established by governmental authorities and because energy tax credits permitted other industries are not allowed to electric utilities, many of the usual incentives to take commercialization risks are not available to our industry. Thus, stability in federal research programs that help reduce technological risk is particularly important to our efforts. We are not able to make the large investments required for fuel cell commercialization unless we can rely on all participants in the program to play their appropriate role consistently.

Unfortunately, the federal role in the fuel cell program has not been consistent. Each year in the annual budget cycle, the content of the federal program has been completely reconsidered and funding levels have remained in flux until final appropriations action. This instability is in marked contrast to the aggressive Japanese fuel cell program, to which their government has made a long-term commitment. This program is subject to periodic substantive reviews, but not to annual reauthorization. It would be unfortunate if this serious and systematic Japanese effort to develop a fundamentally American technology were to succeed whereas ours failed because we were unable to forge the necessary partnership between public and private sectors.

The Fuel Cell Users Group is uniquely positioned to work with your Administration to develop a cooperative program for fuel cell commercialization within the context of your policies on federal energy and development. We plan in 1982 to work closely with the Department of Energy toward this end, and in the coming months will recommend specific program goals and implementing actions. However, this cooperation will not be possible at the reported funding level for FY 1983, which appears to represent withdrawal of the federal government as a partner in the total effort. As noted in the GAO report mentioned earlier ". . . Individual projects need to be carefully evaluated before they are discontinued. Cancellation of such R&D efforts on any other basis can result in commercialization of promising technology being delayed or lost."

We believe continuation of the federal program at the level of the fiscal '82 appropriations would be a more equitable federal share in view of the commitments being made by industry, and we therefore urge your reconsideration of fuel cell technology in the FY 1983 budget.

We look forward to working with your representatives in continuing an effective partnership to realize the cost, efficiency and environmental benefits of fuel cell power plants for our industry and for the nation.

Respectfully,

William H. Corkran

William H. Corkran, Jr.
Acting Chairman

145. FEDERAL SUPPORT FOR FUEL CELL COMMERCIALIZATION—Rural electric cooperatives have great interest in the development of fuel cell power plants because of significant advantages not found in conventional generating equipment. These characteristics show promise of economic benefits for the consumer, improved service availability, conservation of fuel resources, and a cleaner environment.

Commercialization of fuel cell power generation is so near, yet so far from being achieved. The phosphoric acid fuel cell has been developed to the point of large-scale demonstration at the power plant level. But the continued commercial development of the fuel cell continues to need major federal government financial support.

The next step in establishing commercial viability involves broad exposure in the market environment. The risk of proceeding as a strictly private investment has become unacceptably high, due to the economic climate and changing and unpredictable government involvement in utility decisions.

It is both essential and proper that the federal government assume a significant portion of the financial burden. We urge the Congress through the appropriate federal agency to support a comprehensive integrated program with all segments of the utility industry to provide a basis for commercial fuel cell power plant development for electric utility service. (J-1981)

Adopted by the membership of the National Rural Electric Cooperative Association at the annual meeting in Atlanta, February 10, 1982, attended by more than 9700 people.

MR. GARY TIPPS

Mr. Tipps has been with the Tampa Electric Company for eight years, and has been the Senior Engineer in the Generation Planning Department for the last three years. He received his degree in Mechanical Engineering from South Florida University, and is a Registered Professional Engineer with the State of Florida.

His current job responsibilities include the preparation of the company's generation expansion study. He is the company specialist of future alternative generating technologies, and has prepared testimony in proceedings concerning the fuel use act, rate cases, and unit need hearings.

Mr. Tipps is currently serving as Chairman of the Generation Task Force of the Florida Electric Power Coordinating Group, which is an inter-utility coordinating organization of thirty-seven member utilities in the State of Florida.

MR. BOB ROSS

Mr. Ross is Director of Engineering for Seminole Electric Cooperative, Inc., a generation and transmission cooperative providing power to eleven distribution cooperatives in Florida.

His job responsibilities include planning of power supply resources, design and construction of transmission facilities and overseeing the utility's environmental programs.

Mr. Ross has 31 years of experience in the operation and planning electric power systems. He is a graduate of Indiana University, is a member of the Institute of Electrical and Electronic Engineers (IEEE), and is a member of the Management Committee of the Fuel Cell Users Group of the Electric Utility Industry, Inc.

MR. RICK BREITMOSER

Mr. Breitmoser has been with Jacksonville Electric Authority since 1971. He has a BS in Chemistry and a BS in Chemical Engineering, both obtained from the University of Florida. He is a Registered Professional Engineer with the State of Florida.

From 1971 through 1978 Mr. Breitmoser was in the Power Engineering Division with JEA, and has been the Chief of Research & Environmental Affairs there for the past three years. His job responsibilities include promoting Research and Development for the utility, as well as managing all the environmental affairs of JEA. This includes the licensing of a proposed coal-fired plant.

Mr. Breitmoser's experience has included the administration of contracts for installation of flume gas control instrumentation, extensive boiler-related experiences, ambient air monitoring system experience, as well as a strong knowledge of air pollution control technology.

Mr. Breitmoser is currently engaging in the study of fuel cell application and other fossil fuel energy alternatives such as photo-voltaics.

Mr. **Fuqua**. Thank you, Mr. Ross.

Now we will be pleased to hear from Mr. Eric Reichl.

STATEMENT OF E. H. REICHL, CONSULTANT

Mr. **Reichl**. Thank you, Mr. Chairman. I certainly appreciate the privilege to present these very brief comments to this committee, and I should add that I speak solely for myself and not for any organization, although I am a member of the Energy Research Advisory Board.

The subject for discussion is the fiscal year 1983 budget for fossil energy, and this budget implies, of course, a sharp and truly fundamental change in Government policy. And I will probably differ from most other witnesses this morning: I am in full agreement with this change in course.

The 1983 budget, which implies a reduction of over 90 percent from the 1982 high, eliminates many projects and activities. Let me say right now, a rather obvious thing; namely, that this does not mean necessarily that they have no merit; there is some merit in almost all these activities, and the question is whether these are best handled by way of the Government or otherwise.

In order to judge the merit of the proposed cut in the budget it may help to recall briefly, at least, how it grew to the enormous proportion in the first place.

I think that historically the private fossil energy business has always made the necessary R. & D. effort to keep the United States adequately supplied with solid, liquid, and gaseous fuels. The picture was first distorted in the postwar period by Government intervention when the coal industry had lost all of its market except the generation of electric power and found itself confronted suddenly by a multibillion dollar Government R. & D. program aimed at its last remaining business. You may recall at that time nuclear energy would be so cheap that we wouldn't install any switches. It was cheaper to keep the lights burning.

In its desperation the coal industry asked for Government R. & D. help and that resulted in the Office of Coal Research in 1962, and I think this was the first time that the private sector has asked for Government R. & D. help; and, incidentally, this was vigorously opposed by some of the private sectors that were not in the coal business.

There was one earlier period when the Government, on its own initiative, made a major foray into fossil energy R. & D. This was the $80 million program under the Office of Synthetic Fuels, which was then in the Department of the Interior, spent roughly between 1945 and 1955. Now, even if you adjust the dollars to their present value that figure is dwarfed by the present DOE expenditures although the Office of Synthetic Fuels was a big job in its day. Now, that earlier program, I would say, was unsuccessful. It did little more than reconfirm what was already known, and this should have been a warning sign but it was ignored.

The new Office of Coal Research established in 1962 had a minimum budget for its first 10 years of existence. Between 1963 and 1973 the total spent was roughly $116 million, about $12 million annually. Obviously this was not enough to solve the coal industry's problems which had been the purpose of creating OCR in the first place.

Now all this changed quite dramatically in 1973 with the oil embargo when a special study was launched and a report issued at that time by Dr. Dixie Lee Ray, who was then head of the Atomic Energy Commission, to propose a crash R. & D. program of major proportions. She suggested $2.5 billion over 5 years, and that was the beginning of ERDA which was later merged into DOE. Between 1973 and now we have actually spent not just $2.5 billion but $4.6 billion on fossil energy R. & D., and the rate had ballooned by 1980 to a figure of $1.135 billion annually.

In an absolute sense you could argue that that figure is not really excessive. It is small compared to the R. & D. funds spent on nuclear power, and it is small, all in all, in relation to the total fossil energy bill in the Nation. But the question is; Is it helpful? I believe the results are mixed and I would like to make a few very brief observations on this.

First of all, the ERDA/DOE expenditures in fossil energy produced a very large group of engineers and scientists of many varied skills who are now familiar with coal and its use; and I mean all aspects of it, from burning to liquefaction, fuel cells, whatever—including operation and environmental impacts of these operations. This is a very major contribution for which we should be very happy. The United States did not have such a cadre of experts before, but it now has it; and I think that job is complete.

On the negative side, I think the problem that we have noted all morning is that the large DOE R. & D. budget has served to dry up a large part of the private research which might otherwise have been spent in this area. As long as the DOE is potentially available, obviously, that discourages private funds. What it means is that the DOE program in this area became the national program—there was very little outside of it. And I think that the management of this, with the lack of a corrective internal competition that is characteristic of free market, was lacking. So, what we see is a selection of projects and programs that inevitably must involve some political as well as technical considerations. That is inherent in publicly funded jobs, and this has led to some inefficiencies and distortions in the selection and conduct of the work.

The question now, it seems to me, is not whether the $4.5 billion that was spent was well spent or useful. You might say obviously some of it was well worthwhile, some of it was not as it is in any R. & D. program. The question now is whether it is necessary to continue it at this rate, and my conclusion for that was: No.

Just last I week attended a meeting of the International Energy Agency which served to review the status of coal liquefaction. This was, of course, one of the goals, one of the key goals, of the DOE program that was so costly. I could give you the conclusion that that group reached; in a nutshell what it says is that as you know there are two basic routes of liquefaction, indirect and direct, which you certainly are familiar with by now. Of these the former has now reached commercial readiness, particularly with the construction of a SNG project in North Dakota, the commercial one, with a 20,000-barrel-a-day equivalent capacity.

This technology is now being transferred to the United States. There are alternative versions of this indirect conversion with different gasifiers, different catalysts to convert the raw gas, and these are reaching commercial status, building full-size trains right now, both here an abroad. There is no need for further commitment in this area.

I might add that in spite of a very broad R. & D. program, including an entire set of new pilot gasification units here, the major advances, with some exceptions, obviously, were not made in this country.

Now, while we are talking about indirect conversion I think implicitly this includes not only coal liquefaction but coal gasification as well. The direct liquefaction, as was mentioned earlier today, is not in this position. The so-called hydrogenation technology is not commercial; no units of modern design of full scale exists anywhere. But here you might say the DOE program has now essentially completed an important major phase of this. There have been large scale pilot plants in the 200-ton-per-day range, built and run—maybe not as long as you would like to, but they have been run. Operators have been trained, and in one case a full commercial plant designed for 6000-ton-a-day train has been essentially completed.

And so I would say the data base is in hand to proceed with construction of the first commercial size hydrogenation unit, and I believe the proper way to bring this about is not the way of a super-size DOE program but by providing incentives to the private sector to do so.

In the United States one presumes that this is the specific job of the Synthetic Fuels Corporation. Other countries will use different mechanisms—it was quite interesting to compare the view of the Japanese and Germans in this connection. But there is no doubt that whatever it is Government help has to be supplied, some incentives have to be put in place or this will not happen. There is no location anywhere in the world where coal liquefaction is an attractive commercial up-front proposition.

On the other hand, it is emphatically not an R. & D. problem today. You just cannot change the fundamental economics of converting coal to either liquid or gas in the near or middle term.

Now, when it comes to conversion of coal to electricity—we heard a lot about that today already—certainly coal is the mainstay of that form of energy in the future, and as you know one of the technological new waves, as I like to call it, has been fluidized bed combustion. And here the DOE has made a significant contribution by creating an interest in the subject. But I think a point has now been reached where the private sector has stepped in and if the promises claimed for this approach allow it will be commercialized without further DOE support. That is my view.

The reduced budget proposed, of course, will still allow for the pursuit of new concepts for fossil fuel on the bench and exploratory scale. There will always be worthwhile things to test, whether this is—as I understand most importantly now—coal cleaning, coal/water slurry behavior in combustion, the simultaneous removal of nitrous and sulfur oxides from flue gas and certain catalytic work.

I think they specifically are the items the budget will concentrate on.

Now, there is finally, I think, a very major need where the Government has to remain active, and that is in the whole environmental area, whatever you want to include in that. I might say a major issue where I am disappointed somewhat in the proposed 1983 budget is in the acid rain area. I think that would have deserved somewhat more attention, although it is included in the program.

Let me repeat very quickly, I support the change of emphasis in the proposed new budget; the $4.5 billion plus that have been spent have produced an adequate set of trained manpower and an ample data base for the private sector to proceed with; and hindsight being 20-20, of course it is a little easier to comment on it, but I will dare to make the statement that I believe the effectiveness in producing this data base and manpower has been low. This is the result of carrying out a technology selection and development program under political auspices. It is not conducive to a very efficient operation; it is very difficult; it is inherent in the system and not the fault of any group of individuals. There were many dedicated, very competent and highly professional people on the DOE staff who did the best they could under the circumstances; but fossil energy R. & D., at least, the circumstances were forbidding at times.

As to the synthetic fuels area I believe it is entirely appropriate for the United States to create a limited synthetic fuel industry at least comprising a set of commercial plans of varying technology, but that is not part of the DOE, that is an SFC job and of course there will never be unanimous agreement of how big it should be or whether it should be done, but I think a small program of this is justifiable.

Thank you very much.

[The prepared statement of Mr. Reichl follows:]

THE F/Y (83) DOE/FOSSIL ENERGY BUDGET

Statement by E. H. Reichl*

March 23, 1982 Hearing before the Subcommittee on Energy Development and Applications

Mr. Chairman:

It is a privilege to present my brief comments to this Committee.[†] The subject for discussion is the proposed FY '83 budget for Fossil Energy. This budget implies a sharp and truly fundamental change in government policy.[†] I am in full agreement with this change in course.

The '83 budget, which implies a reduction of over 90% from the '81 high, eliminates many projects and activities. Let me say right now that this does not necessarily imply that they have no merit; there is some merit in almost all of these activities. The question is whether these are best handled by way of a government R&D program.

In order to judge the merit of the proposed cut in the budget, it will help to recall briefly how it grew to such a huge outlay in the first place.

Historically, the private fossil energy business has always made the necessary R&D effort to keep the U.S. adequately supplied with fossil fuels. The picture was first distorted in the post-war period when the coal industry had lost most of its market except the generation of electricity and suddenly found itself confronted by a multi-billion dollar government R&D program aimed at this last remaining use of coal. You may recall, nuclear power would be so cheap that it would be better to keep the lights burning rather than install switches. The coal industry, in desperation, asked for government R&D help which resulted in the creation of the Office of Coal Research in 1962. This was the first time, I believe, that a sector of the private fossil energy business asked for government R&D. Incidentally, this was vigorously opposed by the other sectors.

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* A brief resume of my experience and specific expertise is attached for your records. Since my retirement three years ago, I have been active as a consultant in the field of synthetic fuels and coal utilization. I am a member of the DOE Energy Research Advisory Board. I appear at this meeting as an independent citizen.

But there was one earlier period when the government on its own initiative made a major foray into fossil energy R&D. This was the $80 million program under the Office of Synthetic Fuels (in the Department of the Interior, spent roughly between 1945 and 1955). Even if one adjusts the dollars to their present equivalent, the figure is dwarfed by that of the recent DOE expenditures, but it was a big program in its day.

That earlier program was unsuccessful; it did little more than reconfirm what was already known. This should have been a warning sign, but it was ignored.

The new Office of Coal Research had a minimal budget during the first 10 years; the total spent between 1963-1973 was about $116 million, only about $12 million yearly on the average. This was not enough to solve the coal industry's problems which had been the purpose of creating OCR in the first place.

But this changed dramatically in 1973 with the oil embargo. In a special report dated December 1, 1973, a crash R&D program was proposed by Dr. Dixie Lee Ray; she suggested expenditure of about $2½ billion on fossil energy R&D over a 5-year period. The goal was to bring about a major switch from oil to coal in a variety of energy markets. Between 1973 and 1981 we have actually spent over $4.6 billion on fossil energy R&D. The annual rate had balooned to $1.135 billion.

In an absolute sense, the figure is not really excessive. It is small compared to the R&D funds spent on nuclear power and small in relation to the total fossil energy bill. But is it helpful? I believe the results are mixed and I would like to make a few observations:

- The ERDA/DOE expenditures have produced a large group of engineers and scientists of many varied skills who are now familiar with coal and its use; with all aspects of it, from burning to liquefaction, from design of equipment to its operation to understanding the environmental problems.

This is a major contribution. The U.S. did not have such a cadre of experts before. But it has now been created; the job is completed.

- On the negative side, the large DOE R&D budget served to dry up a large part of the private R&D funds, some of which at least might otherwise have been spent. Thus, the DOE program became THE program. This eliminated the corrective impact of a free competition, a very important ingredient in keeping R&D properly focused.

- Selection of projects or programs inevitably involved political as well as technical considerations; that is inherent in any publically funded operation. This led to some inefficiencies and distortions in the selection and conduct of the work.

- The question now, it seems to me, is not whether the $4½ billion spent to date was well spent or useful. Obviously, some of it was - - - some of it was not, as it is in any R&D activity.

  The question is whether there is now a need for it to continue. My conclusion, in the main, is NO.

I have just attended a meeting of the International Energy Agency which served to review the status of coal liquefaction. This was one objective on which a big part of the DOE program was concentrated. Of the two basic routes, indirect and direct conversion, with which you are by now certainly familiar, the former has attained commercial readiness. Particularly with construction of the SNG project in North Dakota this technology is being transferred to the U. S.. Alternate versions of it, involving other gasifiers and alternate ways to convert raw gas to transporation fuels are reaching commercial status by building full size trains right now; both here and abroad. There is no need for further large scale government R&D.

Incidentally, I might add that in spite of a very broad R&D program, including an entire set of new pilot gasification units here, the major advances in this area came from abroad, or were developed by U.S. sponsors abroad.

Implicit in this conclusion regarding indirect liquefaction is, of course, conversion of coal to pipeline gas. It, too, is commercially ready.

This is not the case for the so-called direct or hydrogenation technology; no commercial size units of modern design exist anywhere. But here the DOE program has essentially completed an important major phase of R&D and foreign activities are approaching the same status. Large pilot plants in the 200 ton/day class have been built and run; the operators trained and in one case a commercial size plant has been designed in considerable detail; I refer to the 6000 ton/day SRC-I project.

Thus, the database is in hand to proceed to construction of the first commercial size hydrogenation units. I believe the proper way to bring this about is not by way of a supersize DOE program, but by providing incentives to the private sector to do so.

In the U.S. presumably this is the specific job of the Synthetic Fuels Corporation. Other countries use different mechanisms, but there is no doubt that government incentives are required; there is not likely any location, or economic condition, where a coal liquefaction plant is an attractive proposition. But it is emphatically not an R&D problem. No DOE R&D expenditures are able to change the fundamental economics of converting coal to gas or liquid over the near or middle term.

What about converting coal to electricity where coal has a major contribution to make. One of the technological "New Waves" here has been the fluidized bed combustion concept. DOE has helped create interest in the subject, but a point has been reached where the private sector has stepped in and if the promises claimed for this approach are valid it will be commercialized without further DOE support.

The reduced Fossil Energy Budget will still allow for the pursuit of new concepts for fossil fuel processing on bench scale and on exploratory bases. There will always be new ideas to test, whether these are new ways to clean coal or to make coal/water slurries, or to simultaneously remove sulfur and nitrogen oxides from fluegas, or new concepts in catalysts. I gather it is the intent to pursue these specific subjects.

Finally, there remains a critical need for maintenance of a databank related to all the environmental problems associated with use of fossil fuels. A major issue here is the Acid Rain problem. I am somewhat disappointed that DOE has not put a higher priority on work in this area.

In summary, I would like to repeat:

- I support the change of emphasis in the fossil energy R&D Program.
- The \$4½ plus billion spent have produced an adequate set of trained manpower and an ample database for the Private Sector to proceed.
- Hindsight has 20/20 vision but I will stick my neck out and state that the effectiveness of producing this database was very low indeed. This is the result of carrying out a technology selection and development program under political auspices. This is not conducive to an efficient operation. It is inherent in the system and not the fault of any group of individuals. There were many dedicated highly professional people in the staff who did the best they could under the circumstances. But, in fossil energy R&D at least, the circumstances are forbidding.

I still believe it is entirely appropriate for the U.S. to create a limited synthetic fuel industry at least comprising a set of commercial scale plants of varying technologies. The Synthetic Fuels Corporation is a sound vehicle for this purpose, because it envisions an essentially private sector operation, with appropriate risks taken by the private sponsors; but the SFC must reduce these risks to an acceptable level and I gather that is their plan.

There will never be unanimous agreement on the merit of doing this, but by keeping the total scope of such support reasonably limited, say to the equivalent of around ½ million bbl per day, we can create a very adequate insurance against future technological surprise. There is no more cost effective way to do this and I hope it will be completed during this decade.

Thank you for your attention.

BIOGRAPHICAL RESUME: ERIC H. REICHL

Personal:	Born in Vienna, Austria, December 3, 1913 U.S. Citizen - Married, two daughters
Education:	Equivalent degree to MS Chemical Engineer Technische Hochschule - Vienna, 1937
Employment:	
1938	Babcock & Wilcox Company Field Engineer (construction)
1938-1944	Winkler-Koch Engineering Company, Wichita, Kansas (and subsidiaries) Research, plant design - construction, operations
1944-1946	Stanolind Oil & Gas Company, Tulsa, Oklahoma (now Standard Oil of Indiana) Process research on synthetic liquid fuels including tour of duty with U.S. Navy as civilian technician to evaluate German synthetic oil industry - January - June 1945
1946-1948	California Research Corporation (Standard Oil of California) Process Research - Petrochemicals
1948-1954	Consolidation Coal Company - Research Manager
1954-1962	Consolidation Coal Company - Director of Research
1962-1974	Consolidation Coal Company - Vice President, Research*
1974-1978	Conoco Coal Development Company - President (subsidiary of Continental Oil Co) Retired 12/31/78
1979-	Consultant
Professional Associations:	American Chemical Society American Institute of Chemical Engineers Member, National Academy of Engineers Member, Energy Research Advisory Board (ERAB)/DOE Member, Research Coordination Panel/Gas Research Inst. Member, Gov't. Tech. Adv. Comm./USERDA 1973 Chairman, Coal Task Group/National Petroleum Council Energy Study - 1972 Chairman, Coal Conversion Panel/National Academy Energy Study (CONAES) - 1977 Member, Liaison Committee of International Institute for Applied Systems Analysis

*Note: Consolidation Coal Company's research and development program includes pipelining of coal; conversion to liquid, gases and chemicals; sulfur recovery; continuous coking.

Mr. **FUQUA**. Thank you very much, Mr. Reichl, and we appreciate your testimony.

Mr. Kossar, in the termination cost which you are claiming in the chart in your prepared testimony you seek reimbursement for the total energy system. I think it is around $5 million—$3.5 million.

Mr. **KOSSAR**. Correct.

Mr. **FUQUA**. Has this facility or this system been operating at your site several years prior to this project?

Mr. **KOSSAR**. That is correct. That was built by the company and went on stream as a cogeneration system in the early 1970's, and was supplying us electric power and plant steam, and using both liquid and gaseous fuel for the engine.

Mr. **FUQUA**. Has it been depreciated out by your company?

Mr. **KOSSAR**. It was depreciated in that earlier period, that is correct. But our present value—we took a look at this number because this issue came up—the unit, if operating today and not in the process of conversion for the pilot plant, would be earning us close to $1 million a year in saved costs on both our electricity and steam.

Mr. **FUQUA**. But you have had the tax benefit of writing it off?

Mr. **KOSSAR**. Fortunately we were making an income during that period, so we did have that tax benefit. That is right.

Mr. **FUQUA**. You also indicate in your statement that the original contract was two-thirds Government, one-third private in a cost-sharing arrangement, and you indicated that the Government has put in $58 million thus far under that if my arithmetic is correct, and your contribution should be around $29 million.

Mr. **KOSSAR**. Yes; actually the Government appropriations so far total $58 million. The actual investment by the Government in the current activity is about just under $48 million at this point.

Our cost share is on a formula basis, and the total private sector would come to about $28 million on the total program. I should point out that one of the savings to the Government was the availability of that total energy unit which would have otherwise have had to be built for the plant. Our contract provides for restoration and compensation for that unit in the event the project does not go forward.

Mr. **FUQUA**. How much have you contributed to date?

Mr. **KOSSAR**. Literally to date is the total energy unit. The program that we originally laid down had two steps of cost share during the actual R. & D. program itself. One was during construction, and that was the total energy unit. The second was during the operating phase of the plant, and that would be subsequent to another amount that would be paid out of revenues from sale or lease of such units. So the instant situation is those $5.3 million in the current program. Our offer has increased that to $8 million in the construction phase alone, and substantial additions with new private sector money in the operating phase.

Mr. **FUQUA**. That still is short of the $28 million you——

Mr. **KOSSAR**. I should point out that if you look into the source selection document you will find that our cost share was the best one offered of those competitors active at the time.

Mr. **FUQUA**. But you are still short?

Mr. **Kossar**. Oh, yes; in terms of matching the Government dollars; however, the formula that we have provides for that activity. I think the emphasis has to be on what it will cost the Government to finish this program, and that is cut down to quite a substantially lower number than originally expected.

Mr. **Fuqua**. Dr. Mechlin, how would our industrial competitiveness be affected by the proposed Federal withdrawal from the current technology and development activities?

Dr. **Mechlin**. I think you have to look at it country by country. Japan is always the one that becomes the strongest when you make this comparison. The Japanese have a program which in many respects parallels both the content and emphasis of the U.S program. Those Japanese programs are continuing. There have been no questions to my knowledge of any significance raised in Japan about the necessity of reducing their R. & D. in energy because of their budget deficits, which apparently are even greater in proportion to ours. It is the general Japanese intention of not only utilizing its R. & D. efforts in its domestic markets but also to use those R. & D. efforts in its overseas markets. So there is in that country no indication, no signal, that they have either changed their national ambitions or have changed their program. And in several decades, if the administration carries out its present intentions I think the probability that we will be importing energy generation technology and equipment, just as we are importing automobiles, is quite high and increasing.

A number of other countries do have significant projects; South Africa, for example, is making an investment compared to their gross national product which dwarfs ours. The French are making major investments, particularly in nuclear energy. They intend to generate more than half of their electrical energy by nuclear means by the end of this decade. And I think our performance in this very short-range reaction to our budget problems is distinctly different from our international competitors'.

Mr. **Fuqua**. Do you think that Federal funding for long-range and Federal role for long-range energy R. & D. is helpful, or does it inhibit investment by the private sector?

Dr. **Mechlin**. Categorically it is helpful. I think the Government has the logical advantage, which I pointed out in my paper, that it does not have the duty to recapture, as a prudent man, the costs of R. & D. simply on the basis of a narrow range of sales as an industrial organization does. It can judge the quality and the prudence of its investments on the basis of the return to the economy and the public as a whole, and that difference in viewpoint does not dry up private investment, but private investment simply would not exist in many cases because the logics of the two are so radically different.

Mr. **Fuqua**. You were here when Mr. Reichl talked that it would be—it was on the negative side that, that it dried up private R. & D. funds.

Dr. **Mechlin**. No, the private R. & D. existed in only very meager quantities in the period pre-OPEC, and there was no demonstration made in his discussion that convinced me that there would have been a change in that performance by the private sector post-OPEC. Indeed, in the market statistics that I showed for the commercial, for the residential sectors, people did not continue to consume energy, they didn't support any active program of R. & D.; they simply learned to do without.

Mr. **Fuqua**. Mr. Ross, is there any significant technical barriers to integrating the fuel cell powerplants into a utility grid?

Mr. **Ross**. No, Mr. Chairman.

As a matter of fact, the fuel cell promises to be one of the easiest new technologies to incorporate into the existing grid. In many cases it—in offsetting investment in large central station capacity, the offset would also improve the investment in transmission facilities. In other words, it would avoid the investment in additional transmission facilities at the same time the dispersed mode of generation is installed close to the consumers and to the load.

Mr. **Fuqua**. Mr. Reichl, we have had other industry witnesses testify that they would not—I am talking about private funding—would not invest in long-term R. & D. where the cost could not be recovered for, say, a decade or more. That has been of concern to us as we try to wrestle with these issues that are before us. I would appreciate your comments on that, and we have got a long background in industry.

Mr. **Reichl**. I don't believe I differ with Dr. Mechlin's point when you think about long range basic research. I think in that area the Government does have a role, and it is certainly within the fiscal year 1983 budget to continue that activity. If I recall, the general science section of the budget is $500-some million or more. What has brought about the big change in the fossil energy budget, which is today's hearing subject, is the elimination of the near-term large facilities. On that score I do stand on my case, that if it wouldn't be done by the Government it would have been done by private industry. Not only this, some of the more significant advances in it were done abroad without the U.S. Government.

Mr. **Fuqua**. Mr. Volkmer.

Mr. **Volkmer**. Yes, thank you, Mr. Chairman.

I have a few questions along these lines of trying to figure out what will be done by the private sector and what won't be done if the Government is not there.

Mr. Kossar, are we going to see—if we get out of pressurized fluidized bed altogether are we going to see from industry in this country a continuation with this program?

Mr. **Kossar**. I don't expect that, and the reason is based on experience. We spent since October of last year, which is when we got our first signal that there would be another disengagement by DOE from PFB, since that time we have been aggressively trying to find additional private sector involvement, and what we found is that the interest was involvement only in the test phase but not in completion of construction. Mr. Yeager made that point here this morning, that if that pilot plant is completed, why then, EPRI sees a role for itself in the use of that plant for very specific testing that they and we recognize as central to the PFB issue.

So, all I can say is our efforts have found additional interest in tests but not in completion of construction. I think one reason for that is that what the industry faces subsequent to the pilot plant is a demonstrator plant. Now, if you find a good site with certain equipment already in place the demonstrator will cost $150 million and up. That is a big number by any measure and something that the private sector is ready to face, but it can't start at this point and begin placing the bet in quantity this early. At least that is what we found, and I think that is the basis for EPRI's position, too.

Mr. VOLKMER. Isn't that basis to where we are in the fuel cells, Mr. Ross?

Mr. ROSS. There is no question that the basis for interest and for continuing support of fuel cell must be based on a developing technology base. And I believe this can only come about through the continued participation of the Department of Energy in the programs which have been ongoing. I think the utility industry, which I represent, would be very reluctant to invest significant amounts of their funds in high-risk technologies at this stage.

Mr. VOLKMER. All right.

Now, Mr. Reichl, what are you going to say about that? I can mention others, too, but there are just two right there at the table.

Mr. REICHL. It is a good one to talk about. I believe, and maybe because—you can correct me—I believe American Electric Power, one of the largest utilities in the country, is supporting a pressurized fluid bed development with its own funds, and abroad oddly enough, and is planning to proceed with it here if that first step succeeds.

If you do not get private support, I think one of the questions one needs to ask always is whether the reason you don't is indeed the lack of financing or whether the merit of the case is questionable.

Mr. VOLKMER. Well, I can understand that last statement, then. Some of us wonder where all this is going to lead us, and we know, like you said in your statement, that some of these funds undoubtedly didn't lead to anything accomplished.

Mr. REICHL. They don't in the private sector, either.

Mr. VOLKMER. But I am sure that we have gone a lot further than where we were in the several years past.

I would just like to comment, Mr. Reichl, very briefly on how I view some of the things of what this Government has done in history in energy, and I will go back to your statement that an earlier program was unsuccessful, that is your 1940's and early 1950's program. We have heard hydrogenation was already known, but it wasn't known in this country.

Mr. REICHL. Well, it was known in this country.

Mr. VOLKMER. We have done hydrogenation? We have never done hydrogenation before.

Mr. REICHL. Well, we have done hydrogenation. In fact, the Bureau of Mines had an excellent program under its lead scientist, Dr. Storch, at the time who was the world leader in this field. And we haven't done it on the scale——

Mr. VOLKMER. I mean before 1949 or 1948, we have done hydrogenation?

Mr. REICHL. Yes. Oh, yes.

Mr. VOLKMER. On a private plant?

Mr. REICHL. Dr. Fieldner, who was the head of the Bureau of Mines, started this program and had assigned Dr. Storch as the key man who was around at that time.

Mr. VOLKMER. Was that in Pennsylvania?

Mr. REICHL. Yes; it was done in Pittsburgh in the lab.

Mr. VOLKMER. But that was very small scale, was it not?

Mr. REICHL. That was small scale. The program that I mentioned that I said was actually unsuccessful was in fact a 60-ton-a-day hydrogenation pilot plant in Louisiana, Mo.

Mr. VOLKMER. That is right, my district. That's why I know a little bit about it.

Mr. REICHL. Right.

It was a very exciting operation, but it did not create information that was in any way new, and that was the bottom line.

Mr. VOLKMER. But they were developing it further?

Mr. REICHL. No; it was basically a copy of a German plant—in fact, it was not only a copy, it was German equipment that we had brought over in the war.

Mr. VOLKMER. A lot of German know-how, scientists.

Mr. REICHL. Yes.

Mr. VOLKMER. But that is what I mean, and that was basically on——

Mr. REICHL. It was hydrogenation.

Mr. VOLKMER. But the sum of this is we stopped that just like we are stopping—what are we stopping now? In the 1950's we stopped the process.

Mr. REICHL. Well, you look back—of course, I was involved in coal processing at the time. It was a dark period for someone like us in the coal business. But that was stopped when the Middle East opened up. There was literally no way to overcome the enormous amount of oil available suddenly in the world. It eliminated any justification for coal synthesis in 1950.

Mr. VOLKMER. Are you saying then that the processes that we have learned and are testing with the certain coals, like the H-coal plant, has not taught us anything?

Mr. REICHL. Oh, it has. On the contrary, I said it did, and what I am saying is an important phase of the DOE job is complete and that is part of it. I think that is one of the key items in it. It was well done; it was well spent; we have trained people. The question is do we need to continue at this point.

Mr. VOLKMER. Well, there is always an argument another year of operation might be helpful.

Mr. REICHL. It is difficult to disagree with that. Is it necessary?

Mr. VOLKMER. Well, to some of us here—just a minute, Mr. Kosser. I guess we have to go vote.

Mr. KOSSAR. I just wanted to make mention about American Electric Power Corp.

I think that is misunderstood by Mr. Reichl. Well, since it is close to our business we know a lot about it, and their headquarters are right across the river in New York City and we exchange information and we are good friends. They wrote a very strong letter of endorsement for the pilot plant to DOE for a very good reason, and that is that the program under way in Europe first had Swedish Government support, and, second, the first attempt at rotating tubine tests will be a very limited type of activity over there, and they see the pilot plant, as does EPRI, as a very central, necessary facility for solving and evaluating fully those turbine problems that exist.

Thank you.

Mr. **Volkmer**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, Mr. Volkmer.

I want to thank our panelists for being here today, and we appreciate your testimony.

The subcommittee is adjourned until tomorrow morning.

[Whereupon, at 12:12 p.m., the subcommittee was recessed, to reconvene Wednesday, March 24, 1982, at 9:30 a.m.]

FISCAL YEAR 1983 DEPARTMENT OF ENERGY BUDGET REVIEW

(Fossil Energy and Basic Research)

WEDNESDAY, MARCH 24, 1982

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
Washington, D.C.

The subcommittee met, pursuant to recess, at 9:35 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Present: Representatives Fuqua, Volpe, Volkmer, Nelson, Dunn, Sensenbrenner, Skeen, Walker, and Schneider.

Staff present: Martha Krebs, staff director; and William T. Harvey, minority counsel.

Mr. Fuqua. The subcommittee will come to order. Without objection, photographs and recordings will be permitted.

Opening Statement of Hon. Don Fuqua

This morning we will complete our hearings on the fiscal year 1983 DOE Fossil Energy R. & D. budget. We will receive further testimony from witnesses with interests in Fossil Energy R. & D. programs.

Yesterday we heard near unanimous support from industry witnesses for a continuing and strong Federal role in energy R. & D.

Today we expect our witnesses to provide additional insight into the need for government funding of fossil energy R. & D.

Our first witness will be the Honorable John Murtha, Congressman from Pennsylvania, who will share with us his views on the proposed DOE program.

Following Congressman Murtha, in our first panel will be: Mr. Davis Webb, Vice President of Washington Operations, Gas Research Institute; Mr. George Lawrence, President of the American Gas Association; Dr. Bernard Lee, President of the Institute of Gas Technology; and Dr. George Roberts, General Manager of Commercial Development, Air Products and Chemicals.

Our second panel will consist of: Mr. William Podolny, Vice President and General Manager of Fuel Cells Operations, United Technologies; Dr. James Gumnick, Executive Vice President of the Gulf Universities Research Consortium; Mr. Robert Roach, Director of the Synthetic Fuels Project, Environmental Policy Center; Mr. Tobias Anthony, Director of Business Development and Washington Affairs, Research-Cottrell; and Dr. Joseph Yancik, Vice President of Research, National Coal Association.

Now, I welcome Congressman Murtha.

STATEMENT OF HON. JOHN MURTHA, A MEMBER OF CONGRESS FROM THE STATE OF PENNSYLVANIA

Mr. Murtha. Mr. Chairman, I appreciate being able to appear before this subcommittee, and I want to express my appreciation to you and this subcommittee for your funding in the past and recognizing the importance of fossil fuels. The country owes you a debt of gratitude for your interest and for your foresight in seeing the need for R. & D. funding in this very vital field.

I have a great concern that the decisions that are being made will have a long-range detrimental impact on this country. I am hopeful that my testimony will change the minds of some of the members of the subcommittee, and we will be able to continue the progress under your leadership that we have had in the fossil fuel energy.

The one particular project that I want to talk about is the Homer City Coal Gasification Plant and the $13 million in funding which has been zeroed according to the administration's budget. I am submitting for the record a detailed report on the past and future work at the plant located in the district which I represent.

In summary, I would like to answer three key questions about the plant. First, is the work being done at this plant important? The answer to that is that if Congress is serious about the commitment made to develop an American synthetics fuels industry, then this plant's work is vital.

I strongly believe in the synthetic fuels commitment. I think it is vital to the future economic and energy stability of the Nation.

At this plant, basic, fundamental, and new research is being done that will cut the time it takes to develop this industry. The basic research is on gasification and converting coal to high-pipeline-quality gas. While there is a combination of research done on western coal, virtually no research has been done on eastern coal.

Coal has been around for so long that we sometimes think we know everything we need to know about it. We do not. Last year, when I visited the Pennsylvania State University, experts in their mining school outlined for me research going on that still seeks to define the basic properties of coal. This is after centuries of its use. We still don't have all the answers to gasification which is so vital to using coal in synthetic fuels.

That is the research going on at Homer City, and it will speed our conversion to synthetic fuels.

The second key question is whether the work will be done by private industry. Speaking about this plant, the answer is, "No." If Government funds are withdrawn, the research will stop, even though we have worked diligently trying to find a private enterprise that would be interested. If that happens, the synthetic fuels process will slow.

Now, let me add that I have joined with community leaders in vigorously pursuing this. The community leaders realize the importance, and they are obviously interested in the jobs in the district, but they have done everything possible trying to attract private industry to this plant. In my estimation, if we don't continue to fund it at least for another year, there is a possibility that we will have a substantial—well, we will have no research being done on the eastern coals. They need this extra year to concentrate on the eastern coal.

The final question is, what happens if we do not fund this plant? The direct simple answer is our energy effort will take a giant step backward, and the move toward synthetic fuels using coal will suffer. In turn, the longer it takes to move toward synthetic fuels, the longer the coal industry remains depressed.

Mr. Chairman, let me say this—and I will submit the rest of my statement for the record—I was talking to a number of people who are experts in research, and we were concerned about the basic thrust of what is going on. I know you have been in the forefront of realizing the importance of research and development over the years. I am concerned, if we don't as a Congress continue our research effort, that our children and their children will suffer substantially.

In the area of synthetic fuels, I cannot understand why the administration doesn't realize the significance. The minute we have an economic upturn, I am concerned that we will again have an energy shortage because of the inadequate world supplies of oil. The answer to this is that the synthetic fuels will make us less dependent on the Middle East oil.

[The prepared statement of Mr. Murtha follows:]

Testimony of Representative John P. Murtha, before the Subcommittee on Energy Development and Applications, Committee on Science and Technology - March 24, 1982

Mr. Chairman. Committee Members.

I greatly appreciate this opportunity to testify in support of $13.0 million funding for the Homer City Coal Gasification Plant, which has been recommended for $0 funding in the 1983 Department of Energy budget. For the Members' information, I am submitting for the record, a detailed report on the past and future work at the Plant located in the district I represent. In a brief summary, however, I would like to answer three key questions about the plant.

First, is the work being done at this Plant important? The answer to that is that if Congress is serious about the commitment made to develop an American synthetic fuels industry, then this Plant's work is vital. I strongly believe in that synfuels commitment. I think it is vital to the future economic and energy stability of the nation. At this plant, basic, fundamental, new research is being done that will cut the time it takes to develop this industry. The basic research is on gasification in converting coal to high pipeline quality gas. While there's a combination of research done on western coals, virtually no research has been done on eastern coal.

Coal has been around so long, that we sometimes think we know everything there is to know about it. We don't. Last year when I visited the Pennsylvania State University, experts in their mining school outlined for me research going on that still seeks to define the basic properties of coal -- this after centuries of its use. And we still don't have all the answers to gasification, so vital to using coal in synthetic fuels. That's the research going on at Homer City. It'll speed our conversion to synthetic fuels.

The second key question is whether this work will be done by private industry. Speaking about this plant, gentlemen, the answer is "no." If government funds are withdrawn this research will drop. If that happens, the synfuels process will slow. Now let me add, that I have joined with community leaders in vigorously pursuing such private industry investment. We don't expect the Federal Government to fund this plant forever. But the funds are not there now; in this tight economy, that's no surprise. On a larger scale, let me add from my perspective on the Interior Appropriations Subcommittee, that I have found the Administration's principle running throughout this budget -- that private industry will take over basic research -- is simply unfounded optimism. The fact is that if the Government pulls out from basic energy research, it won't be done, and when we enter the next energy crisis, we'll all wonder why we've failed to make more progress.

The final question is, what happens if we do not fund this plant? The direct, simple answer is that our energy effort takes a giant step backward. Our move toward snythetic fuels using coal will suffer. In turn, the longer it takes to move toward synthetic fuels, the longer the coal industry remains depressed, the longer we remain dependent on foreign oil instead of our abundant coal. Without this plant's work, I do not deny we will still eventually have synthetic fuels -- for that day must surely come. But it will not come as quickly, more miners will be unemployed for longer, and another plug to help fill future energy cut-offs and supply shortages will not be there. Budget cuts that mortgage our future and delay our progress are neither wise nor fiscally sound.

I close by noting that independent engineering firms under contract to DOE have concluded that no other facility is available where this valuable engineering data can be obtained for design, construction, and operation of future synthetic fuel plants.

Against the measure of what this plant can provide for America's energy and economic future, I believe it is a very wise continuing investment, and I urge the Committee to approve the $13.0 million authorization for its continuation.

Mr. **Fuqua**. Thank you, Mr. Murtha.

As I understand it, in fiscal year 1982 we authorized $13 million, I think, for this project. It has been zeroed at this time. Is $13 million again what is needed?

Mr. **Murtha**. That is what is needed, Mr. Chairman. I assure you on the appropriations side that if you are able to see your way clear to authorize that money, we will see if we can appropriate the money for it.

Mr. **Fuqua**. How many more years will this be necessary before this test is completed?

Mr. **Murtha**. Well, we have a number of estimates. We think that 1 more year is the answer. Now, that can change. As you know, in research they run into problems and sometimes it take a little longer than you anticipate.

Mr. **Fuqua**. Yes.

Mr. **Murtha**. However, we think that another year is a key. However, if the economy picks up, maybe we can find a private industry to take it over, and we won't have to mothball it which would also be very expensive——

Mr. **Fuqua**. Yes.

Mr. **Murtha**. And we would lose all those technical people who have been working there.

Mr. **Fuqua**. You mentioned that industry had not indicated any desire to do this type of research. Does it have an industry interest as far as commercialization?

Mr. **Murtha**. I probably gave you the wrong impression there. I think that industry does have a desire to do it, but I think that it is a matter of money available to industry.

Mr. **Fuqua**. And it is possibly the risks?

Mr. **Murtha**. Yes. That's right. It is a combination of both: the risks and the money available and the fact that they have to plan long range, too. We have had a number of contacts with the help of your committee in private industry. It has come down to the fact that the risks and the money available both create a problem.

Therefore, the better the economy is, the more chance that we have for private industry being attracted to this. The farther down the road the synthetic fuels industry goes, the more favorable it looks, the better is the chance that we have.

Mr. **Fuqua**. Well, how about the possibility of a cost-sharing program with industry where they pick up part of the tab and the Government picks up part of it?

Mr. **Murtha**. Well, I could not—I don't know that we could find anybody fast enough to implement it this year, but that is certainly a good recommendation.

Mr. **Fuqua**. Yes.

Mr. **Murtha**. I will take that back and see if we can do anything there.

Mr. **Fuqua**. You know, we are, in my opinion, this year about $200 million short just in the fossil area alone to bring it back up to much less than a bare-bones budget. That is a very difficult situation that we find ourselves in, particularly as it relates to fossil.

Mr. **Murtha**. Well, Mr. Chairman, I realize the tremendous——

Mr. **Fuqua**. That doesn't even get it back up to the fiscal year that we are in now.

Mr. Murtha. Yes. Well, there is nobody who has had more impact on the future of this country than this committee under your leadership. I think that you are pointing out a problem that I see, coming from an area that has substantial amounts of coal. I just dread the day, in a sense, when economic conditions get better and then we go back to the situation where we had long gas lines because the production of the oil in the Middle East has been reduced or we have become overdependent again.

Now is the time for us to spend this money and try to reduce our dependence on foreign oil, as you well know.

Mr. Fuqua. And the public will say, Well, why haven't you been doing something?

Mr. Murtha. That is exactly right.

Mr. Fuqua. You have had all this time.

Mr. Murtha. That is exactly right. I think that the public understands, but you are exactly right. Down the road, if we run out, they will say, Well, why weren't you stockpiling oil? Why weren't you doing the research and getting those plants on line if we run out in the future?

Mr. Fuqua. Mr. Dunn, do you have any questions?

Mr. Dunn. I share my colleague's concern that as we move toward a-dollar-a-gallon gas we will forget the real crisis.

I have one question. You say independent engineering firms under contract to DOE have concluded that there is no other facility available where this valuable engineering data can be obtained.

I wonder if it might be helpful to the committee if we could have this report in front of us. It would make our case stronger for the administration and DOE if you could provide us with that independent study.

Mr. Murtha. Absolutely. Yes.

[Material to be supplied follows:]

Stea[illegible]s-[illegible]oger

February 22, 1982

The Honorable John P. Murtha
House of Representatives
Rayburn Building
Room 2423
Washington, DC 20515

Dear Congressman Murtha:

In accordance with your recent request for information on the BI-GAS Coal Gasification Pilot Plant, I am enclosing the following items for your disposition.

[redacted]

[redacted]

I assume that this information will provide adequate justification for your meeting with the House Appropriations Committee on March 23, 1982. However, we would be more than happy to meet with you and Bill Harvey at the BI-GAS Pilot Plant to answer any questions you may have or discuss plans in more detail.

Yours truly,

Enoch M. Fox
Plant Manager

EMF:RKY:fed

Enclosures

cc: M. J. McIntosh
C. Scialabba
R. K. Young

P. O. BOX J • HOMER CITY, PENNSYLVANIA 15748 • PHONE (412) 479-9081

BI-GAS PROGRAM INFORMATION
FISCAL YEAR '82 & '83
CONGRESSMAN JOHN P. MURTHA

I. BACKGROUND

A. Development of BI-GAS Process

B. Design/Construction of Five TPH Pilot Plant

C. Overall Program Objectives

D. Brief Description of Facilities

E. Summary of Operations 1977-81

II. PROJECT STATUS

A. Recap Recent Operations History - Emphasize 1981-82

B. Current Work on Eastern Coal - Pittsburgh 8 Seam

C. Plans - Remainder Fiscal Year '82

1. Complete operations with Pittsburgh 8 coal
2. Operate commercial mode/longer runs

III. RECOMMENDATIONS FOR FISCAL YEAR '83 PROGRAM

- A. OPERATE ALL PROCESS AREAS AT DESIGN PRESSURE AND FEED RATES
- B. CONTINUE OPERATING GASIFIER WITH SELECTED COALS
  1. PENNSYLVANIA, ILLINOIS, WESTERN LIGNITE
- C. INTEGRATE ALL PROCESS UNITS ON PLANNED SCHEDULE
  1. FINAL PROOF OF BI-GAS CONCEPT
  2. PROOF OF OPERATION - ALL UNITS
  3. OPERATION IN COMMERCIAL MODE
- D. PLANS TO CONVERT PLANT TO ENTRAINED-FLOW GASIFICATION TEST FACILITY (EFGTF)
  1. REFER CONCLUSIONS AND RECOMMENDATIONS BY DOE AND OTHER ENGINEERING FIRMS

IV. RATIONALE FOR CONTINUING PLANT OPERATIONS IN FISCAL YEAR '83

- A. PLANT OPERATIONS DURING 1981 AND 1982 MOST SUCCESSFUL
  1. NO SPECULATION
  2. MAJOR PROBLEMS OVERCOME

B. INCREMENTAL FUNDING REQUIREMENTS SMALL COMPARED TO TOTAL EXPENDITURES

C. PROCESS WILL BE WIDELY ACCEPTED AFTER TESTING LOW, MEDIUM, AND HIGH RANK COALS

   1. SIGNIFICANT ECONOMIC IMPACT ON VARIOUS REGIONS

D. RECOMMENDATIONS BY INDEPENDENT ENGINEERING FIRMS UNDER CONTRACT TO DOE

   1. VALUABLE ENGINEERING DATA CAN BE OBTAINED
   2. LIST AREAS WHERE SUCH DATA WOULD BE AVAILABLE FOR DESIGN OF SNG OR SYNGAS PLANTS
   3. FISCAL YEAR '83 OPERATIONS PROVIDES TIME TO CONVERT TO EFGTF

E. INFORMATION OBTAINED FROM EFGTF VALUABLE TO DOE/SFC/INDUSTRY FOR EVALUATION OF COMMERCIAL COAL CONVERSION PROCESSES

F. FINANCIAL JUSTIFICATION - EFGTF

   1. COST FOR ESTABLISHING EFGTF FAR LESS THAN MOTHBALL OR DISMANTLING COSTS
   2. PRESERVES CADRE OF EXPERT ENGINEERING, OPERATIONS, AND CRAFTS PERSONNEL

INFORMATION ON BI-GAS PROGRAM
CONGRESSMAN JOHN P. MURTHA

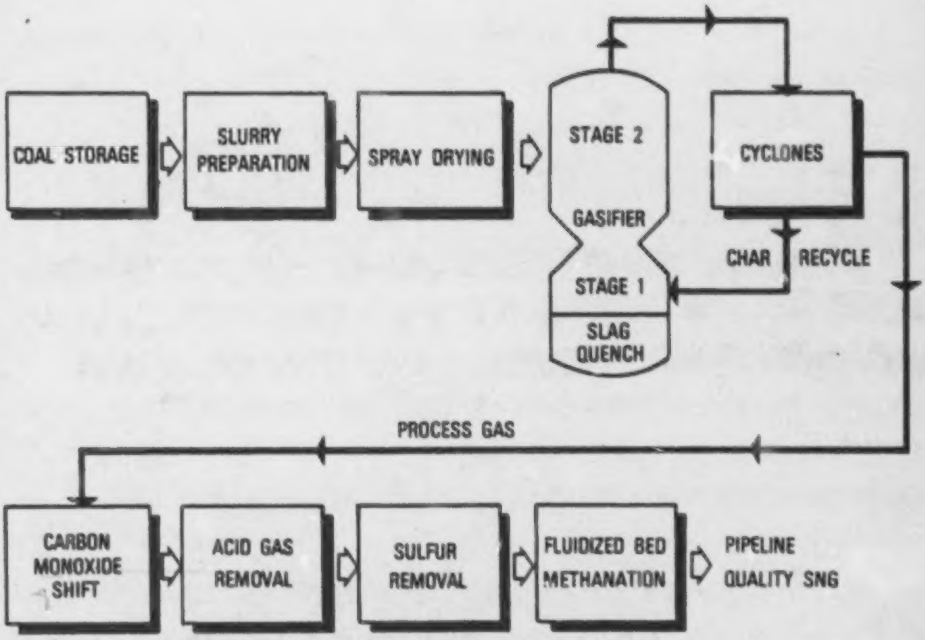
I. BACKGROUND

The BI-GAS coal gasification process concept was developed by Bituminous Coal Research, Inc. initially under contract with the Office of Coal Research, and subsequently the U.S. Energy Research and Development Administration and the American Gas Association. The program at present is fully sponsored by the U.S. Department of Energy.

[redacted] Program plans required that process research and development activities be conducted by Bituminous Coal Research, Inc. to provide information necessary for design of pilot-scale equipment.

After completion of this work in the fall of 1971, Stearns-Roger Inc. was selected by competitive bid to design and construct a five-ton-per-hour BI-GAS coal gasification pilot plant. [redacted]

The pilot plant is [redacted] on a thirty-five acre site near U.S. Route 119 i[redacted] approximately sixty-five miles northeast of Pittsburgh. [redacted]


COAL STORAGE
SLURRY PREPARATION
SPRAY DRYING
STAGE 2
GASIFIER
STAGE 1
SLAG QUENCH
CYCLONES
CHAR
RECYCLE
PROCESS GAS
CARBON MONOXIDE SHIFT
ACID GAS REMOVAL
SULFUR REMOVAL
FLUIDIZED BED METHANATION
PIPELINE QUALITY SNG

BI-GAS PILOT PLANT BLOCK FLOW DIAGRAM

Stearns-Roger is currently the prime contractor for this project and is responsible for detailed research program planning, operation, and maintenance. The U.S. Department of Energy is responsible for overall planning and assists Stearns-Roger in evaluating pilot plant operations.

The [redacted]ption of the pilot plant experimental program in 1977 [redacted] Furthermore, this process was assumed to be generally applicable to various ranks of coal ranging from lignite to high grade bituminous.

[redacted]

II. PROJECT STATUS

Since November 1981, activities at the BI-GAS facility have been restricted due to the availability of funds. Initially, $1,000,000 of FY 82 funds were obligated, an amount sufficient to permit only routine maintenance but no operations for the months of December 1981 and January 1982. In mid-January, an additional $1,500,000 was obligated which would allow for initial operations using Pittsburgh seam bituminous coal.

[redacted]

[redacted] In order to accomplish the latter objective, the plant will be operated for extended periods of time.

III. RECOMMENDATIONS FOR FY '83 PROGRAM

[redacted]. For example, [redacted]

[redacted] It[redacted]

[redacted] all the downstream areas of the plant will be operated. This will include the carbon monoxide shift and fluid-bed methanation process units. Th[redacted]

[redacted]. Independent engineering firms under contract to DOE have conducted such feasibility studies and have supported these recommendations.

IV. [redacted]

The rationale for continuing pilot plant operations during FY 1983 includes several major points. Most important, operations at the BI-GAS facility have been extremely successful during Fiscal Years 1981 and 1982. Therefore, there is no need for speculation because [redacted]

It also [redacted].

With respect to finances, it should be emphasized that the incremental funding required (approximately $12-13M) will amount to only 10% of the total funds already expended on this project.

As mentioned earlier, other [redacted]nder contract to DOE [redacted]ith respect to operations and to condition of equipment. Their[redacted]. [redacted], there is [redacted]n, [redacted]ts. S[redacted], recommendations were made to [redacted]t, [redacted] [redacted]nd ac[redacted]nt[redacted]ials o[redacted]n. [redacted], by operating the plant during FY 1983, a[redacted]

[redacted] At the same time, [redacted]

[redacted], documented incidentally by an independent engineering firm, is that [redacted]

In addition to being an attractive engineering and training center, such a [redacted]y, sponsored by the government, [redacted]

HOMER CITY TESTIMONY - MARCH 24, 1982

MR. CHAIRMAN. COMMITTEE MEMBERS.

I GREATLY APPRECIATE THIS OPPORTUNITY TO TESTIFY IN SUPPORT OF $13.0 MILLION FUNDING FOR THE HOMER CITY COAL GASIFICATION PLANT, WHICH HAS BEEN RECOMMENDED FOR $0 FUNDING IN THE 1983 DEPARTMENT OF ENERGY BUDGET. FOR THE MEMBERS' INFORMATION, I AM SUBMITTING FOR THE RECORD, A DETAILED REPORT ON THE PAST AND FUTURE WORK AT THE PLANT LOCATED IN THE DISTRICT I REPRESENT. IN A BRIEF SUMMARY, HOWEVER, I WOULD LIKE TO ANSWER THREE KEY QUESTIONS ABOUT THE PLANT.

FIRST, IS THE WORK BEING DONE AT THIS PLANT IMPORTANT? THE ANSWER TO THAT IS THAT IF CONGRESS IS SERIOUS ABOUT THE COMMITMENT MADE TO DEVELOP AN AMERICAN SYNTHETIC FUELS INDUSTRY, THEN THIS PLANT'S WORK IS VITAL. I STRONGLY BELIEVE IN THAT SYNFUELS COMMITMENT. I THINK IT IS VITAL TO THE FUTURE ECONOMIC AND ENERGY STABILITY OF THE NATION. AT THIS PLANT, BASIC, FUNDAMENTAL, NEW RESEARCH IS BEING DONE THAT WILL CUT THE TIME IT TAKES TO DEVELOP THIS INDUSTRY. THE BASIC RESEARCH IS ON GASIFICATION IN CONVERTING COAL TO HIGH PIPELINE QUALITY GAS. WHILE THERE'S A COMBINATION OF RESEARCH DONE ON WESTERN COALS, VIRTUALLY NO RESEARCH HAS BEEN DONE ON EASTERN COAL.

COAL HAS BEEN AROUND SO LONG, THAT WE SOMETIMES THINK WE KNOW EVERYTHING THERE IS TO KNOW ABOUT IT. WE DON'T. LAST YEAR WHEN I VISITED THE PENNSYLVANIA STATE UNIVERSITY, EXPERTS IN THEIR MINING SCHOOL OUTLINED FOR ME RESEARCH GOING ON THAT STILL SEEKS TO DEFINE THE BASIC PROPERTIES OF COAL -- THIS AFTER CENTURIES OF ITS USE. AND WE STILL DON'T HAVE ALL THE ANSWERS TO GASIFICATION, SO VITAL TO USING COAL IN SYNTHETIC FUELS. THAT'S THE RESEARCH GOING ON AT HOMER CITY. IT'LL SPEED OUR CONVERSION TO SYNTHETIC FUELS.

THE SECOND KEY QUESTION IS WHETHER THIS WORK WILL BE DONE BY PRIVATE INDUSTRY. SPEAKING ABOUT THIS PLANT, GENTLEMEN, THE ANSWER IS "NO." IF GOVERNMENT FUNDS ARE WITHDRAWN THIS RESEARCH WILL DROP. IF THAT HAPPENS, THE SYNFUELS PROCESS WILL SLOW. NOW LET ME ADD, THAT I HAVE JOINED WITH COMMUNITY LEADERS IN VIGOROUSLY PURSUING SUCH PRIVATE INDUSTRY INVESTMENT. WE DON'T EXPECT THE FEDERAL GOVERNMENT TO FUND THIS PLANT FOREVER. BUT THE FUNDS ARE NOT THERE NOW; IN THIS TIGHT ECONOMY, THAT'S NO SURPRISE. ON A LARGER SCALE, LET ME ADD FROM MY PERSPECTIVE ON THE INTERIOR APPROPRIATIONS SUBCOMMITTEE, THAT I HAVE FOUND THE ADMINISTRATION'S PRINCIPLE RUNNING THROUGHOUT THIS BUDGET -- THAT PRIVATE INDUSTRY WILL TAKE OVER BASIC RESEARCH -- IS SIMPLY UNFOUNDED OPTIMISM. THE FACT IS THAT IF THE GOVERNMENT PULLS OUT FROM BASIC ENERGY RESEARCH, IT WON'T BE DONE, AND WHEN WE ENTER THE NEXT ENERGY CRISIS, WE'LL ALL WONDER WHY WE'VE FAILED TO MAKE MORE PROGRESS.

THE FINAL QUESTION IS, WHAT HAPPENS IF WE DO NOT FUND THIS PLANT? THE DIRECT, SIMPLE ANSWER IS THAT OUR ENERGY EFFORT TAKES A GIANT STEP BACKWARD. OUR MOVE TOWARD SYNTHETIC FUELS USING COAL WILL SUFFER. IN TURN, THE LONGER IT TAKES TO MOVE TOWARD SYNTHETIC FUELS, THE LONGER THE COAL INDUSTRY REMAINS DEPRESSED, THE LONGER WE REMAIN DEPENDENT ON FOREIGN OIL INSTEAD OF OUR ABUNDANT COAL. WITHOUT THIS PLANT'S WORK, I DO NOT DENY WE WILL STILL EVENTUALLY HAVE SYNTHETIC FUELS -- FOR THAT DAY MUST SURELY COME. BUT IT WILL NOT COME AS QUICKLY, MORE MINERS

WILL BE UNEMPLOYED FOR LONGER, AND ANOTHER PLUG TO HELP FILL FUTURE ENERGY CUT-OFFS AND SUPPLY SHORTAGES WILL NOT BE THERE. BUDGET CUTS THAT MORTGAGE OUR FUTURE AND DELAY OUR PROGRESS ARE NEITHER WISE NOR FISCALLY SOUND.

I CLOSE BY NOTING THAT INDEPENDENT ENGINEERING FIRMS UNDER CONTRACT TO DOE HAVE CONCLUDED THAT NO OTHER FACILITY IS AVAILABLE WHERE THIS VALUABLE ENGINEERING DATA CAN BE OBTAINED FOR DESIGN, CONSTRUCTION, AND OPERATION OF FUTURE SYNTHETIC FUEL PLANTS.

AGAINST THE MEASURE OF WHAT THIS PLANT CAN PROVIDE FOR AMERICA'S ENERGY AND ECONOMIC FUTURE, I BELIEVE IT IS A VERY WISE CONTINUING INVESTMENT, AND I URGE THE COMMITTEE TO APPROVE THE $13.0 MILLION AUTHORIZATION FOR ITS CONTINUATION.

Mr. DUNN. Thank you.

Mr. FUQUA. Thank you very much, Mr. Murtha. We appreciate your being here, and we will try to do the best we can.

Mr. MURTHA. Thank you, sir. I am depending on you. [Laughter.]

Mr. FUQUA. Thank you.

Our first panel today will be David Webb who has been here many times, Vice President, Washington Operations for the Gas Research Institute; George Lawrence, President of the American Gas Association; Dr. Bernard Lee, President, Institute of Gas Technology; and Dr. George Roberts, General Manager, Commercial Development, Air Products and Chemicals.

Gentlemen, will you take your seats. We will begin in the order that I called the names off beginning with Mr. Webb. If you have prepared statements and wish to submit them for the record and summarize, it would be welcomed. We will be happy to hear from Mr. Webb.

STATEMENT OF DAVID O. WEBB, VICE PRESIDENT, WASHINGTON OPERATIONS, GAS RESEARCH INSTITUTE

Mr. WEBB. Thank you, Mr. Chairman and members of the committee. I will read the introductory part of my statement and then summarize in view of the constraints of time. I request that the entire statement be submitted for the record.

I am David Webb, Vice President of the Gas Research Institute. It is an independent, not-for-profit, scientific research organization created by the gas industry in 1976 to plan, finance, and manage an expanded and fully coordinated gas-related research and development program for the mutual benefit of the gas industry and its ratepayers. This program is annually reviewed and approved by the Federal Energy Regulatory Commission.

I am pleased to appear here today to discuss my views on the Department of Energy's fiscal year 1983 fossil energy research and development program. Let me state right at the outset that the new emphasis on letting the market govern the development and introduction of new energy sources and technologies is basically sound. Having industry fund the near-term R. & D. and concentrate on the final demonstration prior to commercialization is the correct approach since only industry can effectively introduce new products into the market.

Therefore, while the shift in emphasis outlined in the national energy policy plan is essentially correct, it is such an abrupt change from past Federal energy R. & D. progams that it will require an orderly transition under which the industry can support and pick up the near-term R. & D. previously funded by the Government, with the Government continuing to support the long-term, high-risk research that industry cannot pick up due to normal market incentives.

Therefore, the proposed DOE fiscal year 1983 budget which essentially eliminates all gas-related research in fiscal year 1983, even the long-term, high-risk research, does not provide for such a transition. It is a reversal of the Government's previously stated policy of continuing to fund long-term research. As a result, it jeopardizes some critical projects currently underway in the Federal program.

The gas industry has responded well, I think, to the changes outlined in the national energy policy plan. The gas industry, through GRI, promptly shifted its focus on the new budget priorities, and we will propose to our board of directors for recommendation to the FERC for approval in 1983 an increase of approximately 40 percent in our contract R. & D. budget. This equates to approximately $33 million of which $29 million will be for near-term R. & D. projects previously cofunded with the Government. The gas industry has shifted its research program to pick up the near-term projects in line with the policy of this administration.

Before we get into the specific recommendations, there are a couple of areas I would like to cover. One is the area of determining whether long-term, high-risk research is a national concern. The other area is what is the capability of the gas industry—specifically, the regulated segments of the gas industry—to abruptly respond to sudden changes in Federal policy.

If you consider for a minute that natural gas currently supplies over one-third of the energy used in the industrial sector which is the energy equivalent of 4 million barrels of oil per day, and additionally provides approximately 15 percent of the primary energy in the central power generation market which is equivalent to approximately 2 million barrels per day, that is a tremendous amount of gas which is provided in those sectors.

This market is based primarily on price. Due to previous years' regulation, gas in this market right now has a price advantage. This advantage is rapidly shrinking as we go through the NGPA or any of the other phased-deregulation schemes.

Since the price advantage has primarily relied on past regulation, a strong technology base has not been developed because it was not necessary. Therefore, if the long-term, high-risk research is abruptly canceled, then the technology base to continue to have gas as an important element in the industrial and powerplant market is jeopardized.

Second, the gas industry does not have the quick response time and the necessary resources to rapidly respond to abrupt changes in the Federal energy policy.

I think the Department of Energy's Energy Research Advisory Board put these factors into proper perspective in its report entitled "Federal Energy R. & D. Priorities" released in November 1981.

Let me paraphrase from that report. The report stated:

> The new policy recognizes that private industry cannot be expected to do basic energy research or projects of long-term, higher-risk character, but there are other circumstances in which it would be unrealistic to expect timely and effective assumption by industry of R&D. A little over half our energy finds its way to consumers through the electric and gas utilities, and the utilities are regulated, price-controlled industries, selling their products not at free market prices, but at controlled prices well below replacement costs. The consequences are that consumers of gas and electricity have less of an incentive to conserve and therefore to undertake research on conservation.

The ERAB report goes on further to state:

Both these regulated industries have weak incentives to spend on R&D. If successful, the benefits go to ratepayers; if unsuccessful, the expenditure may be disallowed as "imprudent."

I think the ERAB report is a good statement of the basic differences between the Department of Energy's perception of the regulated segment of industry's ability to rapidly increase research funding and the actual response that industry has.

One key difference, it seems to me, is the difference between the administration and industry in their definition of near-term and long-term when you are really talking about investment decisions.

GRI has gained experience through cofunding with industry approximately 100 research projects in the past 3 years. Our experience has been that industry will only make major resource contributions on projects where they can see a clear chance or an opportunity to earn a return on their investment in the next 5 to 8 years. In other words, if they cannot see a clear path to a commercial product or technology that can be introduced in the next 5 to 8 years, they are not willing to put major financial resources into it.

Therefore, the situation which we have today is that the gas industry is rapidly increasing its research. It is primarily going into the near-term. We are driven there by two reasons. One is the policy of the administration. Second, the rising prices put a tremendous pressure on the gas industry to try to develop end use technology as rapidly as possible.

We have a regulated industry that cannot rapidly respond to changes; therefore, there is a continuing need for long-term, high-risk research sponsored by the Federal Government.

To give you some idea of the sudden shift in the program of the gas industry, the 1981 combined program of GRI, the cooperative program which we had with the Federal Government, and the cooperative program which we had with industry, totaled approximately $165.8 million. This was supplemented by approximately $40 million in additional research by individual gas companies.

Of this segment, $72 million was a cooperative program with the Federal Government. This is in 1981. If you look at the budget proposed by the administration for fiscal year 1983 for gas-related research in the Department of Energy and contrast that with their 1981 and their fiscal year 1982 appropriations, the gas-related research in the Department is declining from approximately $300 million in fiscal year 1981 to only approximately $20 million in fiscal year 1983.

Now, while GRI and the gas industry are proposing to pick up the near-term increase, this abrupt change means many of the long-term, high-risk projects that had previously been cofunded will not be cofunded.

To give you some specific examples of long-term, high-risk research that meets the definition of the administration from their policy standpoint and that will not be pursued by the gas industry through their regulated segments due to market incentives, let me cite the following four examples. One of the major areas is surface coal gasification where the gas industry's recommendation is $35 million. This is one of the most promising long-term technologies which meets the administration's guidelines for Federal funding of energy R. & D. While the technology base underlying surface coal gasification for western noncaking coals is relatively mature, additional research is needed in materials, catalysts, instrumentation, and downstream processing. A significant improvement in the economics of coal gasification could result from improvements in the downstream system since it comprises approximately one-third of the installed plant cost.

Another area is underground coal gasification where the gas industry's recommendation is $10 million. Underground coal gasification offers the largest potential for major reductions in capital investments of the different processes and methods for producing a medium-Btu gas or synthesis gas from coal. However, the high risk associated with this technology precludes industry from developing the technology on its own. Before industry will be willing to make major investments, several basic technology issues must be resolved such as the successful ability to link modules so you know for a fact you can control the combustion and the quality of the gas being produced.

Another major area that is an appropriate candidate for continued Federal support is unconventional natural gas. GRI is increasing its research budget in unconventional gas from $10.8 million in 1982 to $18.1 million in 1983. However, this increase will be focused in the near-term areas we cooperatively fund with industry to do the field tests. The research that will not be continued is the long-term research. Two particularly good examples are the eastern mineback program in the Devonian shales and the multiwell experiment in the lenticular sands in the west.

Just to give you some example of this potential, the National Petroleum Council has estimated that advanced technology could increase by approximately 200 trillion cubic feet the amount of recoverable gas in these formations. The February issue of the Energy Information Administration's Monthly Energy Review has some details on the number of wells drilled in the Devonian shale and the tight sands in response to section 107 incentives. The average production of wells in Devonian shale is only 55,000 cubic feet per day. In the tight sands it is 164,000 cubic feet per day.

If, through this long-term technology, we could just double the production from those wells, if you look at the tremendous part of the United States that they underly, you can see the magnitude of this technology program.

One other area I would like to cover, Mr. Chairman, just before I close, is that the gas industry and GRI are planning to make a major increase in funding for the field testing of the on-site fuel cell systems. We are increasing our budget from $11.3 million in 1982 to $19.6 million in 1983 to offset the loss of DOE cofunding in the field test.

However, there is a continuing need for technology to develop things such as improved catalysts, basic materials, and cell stacks to reduce the cost of phosphoric acid fuel cells.

We think there is a minimum DOE program of $5 million required in this for just the gas industry's interest, and that does not count the larger central power station research.

Overall, the gas industry is recommending approximately $104 million for long-term, high-risk research. It should be noted that this recommendation, while totaling $104 million, is less than the level of funds appropriated by Congress for gas-related research in fiscal year 1982. It is only one-third of the fiscal year 1981 funding level.

It is consistent with projected funding for fossil energy in fiscal year 1983 contained in the Omnibus Reconciliation Act of 1981, and it actually is a reduction of Federal expenditures.

As a final note, Mr. Chairman, these recommendations, I believe, are totally consistent with the actions and the funding priorities established by you and your committee last year in your authorization bill.

Thank you very much.

[The prepared statement of Mr. Webb follows:]

Statement of David O. Webb, Vice President
Policy and Regulatory Affairs, Gas Research Institute
Before the Energy Development and Applications Subcommittee of the
Committee on Science and Technology
United States House of Representatives
March 24, 1982

Mr. Chairman and Members of the Committee:

I am David Webb, Vice President of Gas Research Institute (GRI), an independent, not-for-profit scientific research organization created by the gas industry in 1976 to plan, finance, and manage an expanded and fully coordinated gas-related research and development (R&D) program for the mutual benefit of the gas industry and its ratepayers. This program is annually reviewed and approved by the Federal Energy Regulatory Commission (FERC).

I am pleased to appear before you today to discuss my views on the Department of Energy's (DOE) FY 1983 Fossil Energy Research and Development Program. Let me state right at the outset that the new emphasis on letting the market govern the development and introduction of new energy sources and technologies is basically sound, and having industry fund the near-term R&D and final technology demonstration prior to commercialization is also the correct approach since only industry can effectively introduce new products into the market. Thus, while the shift in emphasis outlined in the National Energy Policy Plan (NEPP) is correct, it is such an abrupt change from past federal energy R&D policy that it will require an orderly transition with critical near-term R&D projects funded under industry sponsorship while long-term research is continued by DOE. Therefore, the proposed DOE fossil budget which essentially eliminates all gas-related research in FY 1983--even long-term, high-risk research--is a reversal of the government's stated policy of continuing to fund long-term research that industry will not fund due to market incentives. Furthermore, implementation of this budget would be extremely shortsighted and not in the national interest.

The gas industry has responded well to the change in government R&D policy as outlined in NEPP. GRI has promptly shifted its focus in line with the new federal budget realities and will propose to its Board of Directors to increase its 1983 contract R&D budget for near-term research funding by approximately $29 million out of a total increase of $33 million over its 1982 contract R&D budget ($84 million to $117 million). However, this proposed increase in GRI funding will be sufficient to fill only the most serious gaps in near-term gas-related research projects previously cofunded with DOE.

In determining the proper federal role and assessing the importance of continued federal sponsorship of long-term gas-related research, it is important to understand the fundamental link between the development of a strong gas technology base and national energy policy. Today more than 85 percent of our energy is derived from fossil fuels, of which gas provides over 25 percent. DOE's NEPP projects that gas will continue to supply over 20 percent of the nation's energy in the year 2000. However, the key to these projections is whether gas will remain competitive in the industrial and power plant markets through technology development. Natural gas currently supplies more than one-third of the energy used by the industrial sector--the energy equivalent of over 4 million barrels of oil per day. In addition, gas provides 15 percent of the primary energy used for central power generation--equivalent to nearly 2 million barrels per day of oil. This market position is largely the result of price regulation which has made gas cheaper than less desirable fuel sources. Retention of this market has remained overly dependent on price, and the key factor to consider for future demand is the shrinking cost advantage of natural gas relative to competitive energy sources. The current deregulation path under the Natural Gas Policy Act (NGPA) or any other accelerated gas deregulation proposal assures that gas prices will increase at a faster rate than the prices of competing fuel sources. The best answer to rising gas prices is a strong technology response with industry assuming responsibility for the near-term R&D and DOE continuing the long-term, high-risk research. Without such a coordinated approach, the potential for major gas market losses to oil imports is great.

Capability to Develop an Adequate Technology Base in the Regulated Gas Industry

The gas technology base necessary to develop cost-competitive supplemental sources of gas and more energy-efficient processes and end-use equipment required to compete with alternative energy sources in the industrial and power plant markets is not established and in place. Although the gas industry is increasing its funding of near-term R&D to offset part of the DOE reductions, the gas industry has limited resources and capabilities to respond rapidly to this challenge compared to its major competition, the petroleum and electric industries.

As presented below, the net result is the current inadequate gas technology base and the limited capacity to rapidly expand funding of R&D.

TECHNOLOGY BASE DEVELOPMENT CAPABILITY

	Petroleum	Electric	Natural Gas
Vertical Integration	Yes	Yes	No
Impact of Supply Deregulation	Very Positive	Possible Threat	Very Negative
Technical Capability	Very Strong	Strong	Limited
Internal R&D Funding Capability	Excellent- Large Corporate Units, High Returns, Large Tax Credits	Limited by Regulation	Limited by Multilayered Regulation
Manufacturer Support	Very Large- Transportation (with DOE), Chemicals, Most End Uses	Very Large- Most Supply & All End Uses (G.E., Westinghouse, etc.)	Limited
Federal RD&D Support	Very Little	Very Large- DOE, TVA	Very Little
Federal Commercialization Support	Limited - SFC	Very Large- Nuclear	Some - SFC DOE (Great Plains)

DOE's Energy Research Advisory Board (ERAB) put these factors in proper perspective in its report Federal Energy R&D Priorities dated November 1981.

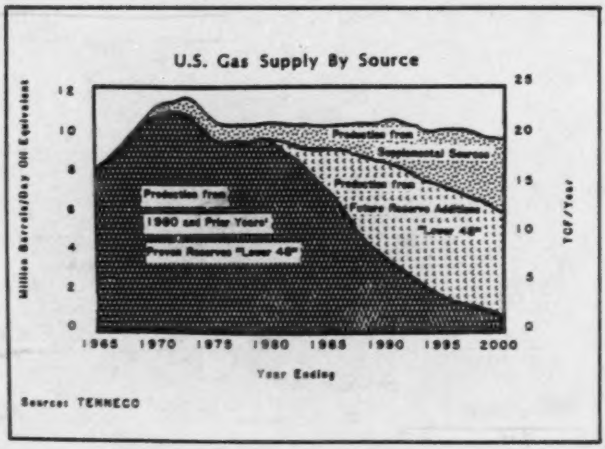
> ERAB is concerned that some energy R&D of great potential significance for the achievement of the Nation's energy goals will fall between federal and industry responsibilities.
>
> The new policy recognizes that private industry cannot be expected to do basic energy research or projects of long-term, higher risk character. But there are other circumstances in which it would be unrealistic to expect timely and effective assumption by industry of R&D . . . .
>
> Some of the markets in which energy is sold are not "free." Oil and coal are the exceptions. A little over half our primary energy finds its way to consumers through the electric and gas utilities, and the utilities are regulated, price-controlled industries, selling their products not at free market prices, but at controlled prices well below replacement cost.* The consequences are that consumers of gas and electricity have less of an incentive to conserve (and, therefore, to undertake research on conservation). . . .
>
> Both these regulated industries have weak incentives to spend on R&D. If successful, the benefits go to ratepayers; if unsuccessful, the expenditure may be disallowed as "imprudent."* In the case of electricity (but not gas), a substantial amount of R&D has been undertaken by large research-oriented equipment suppliers.... A strong R&D response to price signals requires both motivation and capability. In many cases the capability is simply lacking, and while it can be developed in given time, the time required to build a strong research organization is measured in years.
>
> *Emphasis added.

The ERAB report is a good statement of the differences between DOE's perception of industry's ability to rapidly increase research funding due to market incentives and without government sponsorship, and the actual response of industry. One key problem is the total difference between time frames used by DOE and industry to define near-term and long-term regarding investment decisions. GRI's experience gained through the cofunding of many research projects with industry has been that industry will only contribute major resources to projects that have the potential to produce returns on investments in approximately five to eight years. GRI has been very successful in obtaining significant increases in industrial cofunding; however, practically all of the increases have occurred in near-term projects. Industrial partners have stressed that they must be able to identify a reasonable path to a marketable product within five to eight years before commiting themselves to large research investments. With gas prices increasing rapidly, the gas manufacturing and research industry will continue to concentrate on the near-term research due to severe market pressures. If DOE does not continue to support and fund long-term gas-related research, then most of this research will be terminated since industry will not fund this research due to market incentives.

The National Interest in Assurance of Long-Range Supplies

Without adequate continued federal funding for the long-term R&D needed to establish this gas technology base, especially in the areas of least-cost options for supplemental supplies, the most probable result of rising gas prices is increased oil imports due to fuel switching in the industrial market. Increasing quantities of gas production from supplemental sources will be needed by the nation over the next few decades to provide an assured supply to this critical market.

As illustrated in the following chart and based on DOE's recent projections, supplemental supplies of 4.6 Tcf plus nearly 3 Tcf of Alaskan gas and imports will be required in 2000 to meet demand. These supplemental sources will become available at competitive prices only through major improvements in technology. Therefore, establishing the technology base necessary to ensure the orderly development of these potentially abundant or renewable sources of gas is a national priority and requires a continuing federal role since it includes long-term, high-risk research.


U.S. Gas Supply By Source

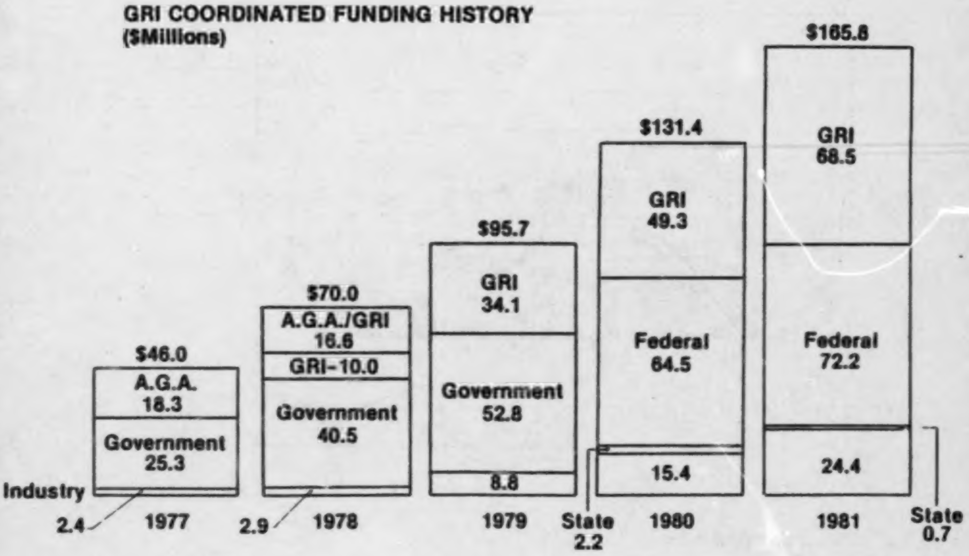
Source: TENNECO

Importance of GRI/DOE Cofunded Gas-Related R&D

In the early 1970s, the gas industry recognized the need for increased funding of its technology base and established the Gas Research Institute. The FERC approved the initial GRI R&D program and five-year plan in March 1978. With the federal emphasis primarily on long-term supply programs, the gas industry, through GRI, concentrated its funding increasingly on the nearer term options to offset as much as possible the impact of rapidly increasing gas prices. High priority was assigned to the residential and commercial markets to address urgent near-term problems such as the need for rapid introduction of high-efficiency gas space conditioning equipment. To cope with this immediate problem and in response to federal research policy, GRI and the gas and manufacturing industries must now assume full responsibility for this research without waiting for possible resumption of federal R&D support.

The growth of the GRI contract R&D program has been impressive, rising from a level of $26.6 million in 1978 to $83.7 million in 1982 while total budgets grew from $27.7 million to $100 million. From its inception, in response to FERC guidance and encouragement by DOE, GRI has relied on extensive federal and industrial cofunding to carry out its research programs. GRI adopted a deliberate strategy to stretch the ratepayer's limited funds by cofunding or cooperatively funding projects of common interest with DOE and other government agencies. This highly successful effort resulted in a combined government and industry coordinated funding level of approximately $97 million in 1981. When combined with GRI funding for contract R&D, this funding amounted to a national institutional gas R&D program of $165.8 million in 1981 supplemented by approximately $40 million from individual gas companies to address issues unique to their service areas.

GRI COORDINATED FUNDING HISTORY
($Millions)



Revised Federal R&D Policy

As I mentioned earlier, the Administration's policy of increasing reliance on the private sector to fund near-term research in conjunction with continued federal funding of long-term, high-risk R&D is fundamentally sound. However, the severe and sudden impact of the proposed DOE FY 1983 budget on GRI and on gas-related institutional research is best illustrated by the change in the size and distribution of the DOE gas-related RD&D budget. The appropriation for gas-related RD&D has declined from $300 million in FY 1981 to only about $20 million in FY 1983 as shown in the following chart.

DOE GAS-RELATED BUDGET
($ Millions)

	FY 1981 Appropriation	FY 1982 Appropriation	FY 1983 Budget Request
Fossil Energy			
Advanced Research and Technology Development	$ 14.5	$ 12.5	$ 4.2
Enhanced Gas Recovery	30.6	8.7	0.0
On-Site Fuel Cells R&D	9.5	5.6	0.0
Surface Coal Gasification	164.9	53.1	10.5
Underground Coal Gasification	10.0	8.3	0.7
Geopressured Methane	35.8	17.3	2.4
Energy Conservation	22.0	10.5	0.0
Biomass	12.5	3.0	2.0
Total	$299.8	$119.0	$19.8

Gas Industry Response to Revised Federal R&D Policy

In response to the NEPP, GRI and the gas industry, with the active encouragement of DOE and the FERC, revised their funding priorities to bring their programs in line with national policy. Through GRI, the gas industry significantly increased its 1982 funding for near-term R&D and will propose another major increase to the FERC for 1983. Out of a proposed GRI contract R&D budget increase of approximately $33 million, $29 million is for near-term projects in such areas as unconventional natural gas, regional and land-based biomass, on-site fuel cells, improved gas appliances, gas heat pumps and more efficient industrial utilization as defined in the following chart. These increases will partially offset the loss of DOE cofunding and coordinated funding for near-term research but will severely limit the ability of GRI to fund long-term gas research.

PROPOSED GRI NEAR-TERM R&D BUDGET INCREASES
($ Millions)

	Approved Budget 1982	Proposed Budget 1983
Unconventional Natural Gas	$10.61	$18.11
Regional and Land-Based Biomass	2.90	5.30
On-Site Fuel Cells	11.35	19.55
Gas Appliances	3.98	6.65
Gas Heat Pumps	7.00	11.30
Industrial Utilization	7.95	11.75
	$43.79	$72.66
	(Increase of $28.87)	

Options for Federal Support of Long-Term Gas Research

The proposed increase in GRI 1983 funding will only be sufficient to fill the gap in near-term gas-related research left due to the severe reductions in federal spending. Therefore, it is essential that the federal government maintain an adequate long-term research program to support the gas technology base. There are important high-risk, long-term, high-payoff areas of gas-related research that continue to require government sponsorship. This research is consistent with the Administration's policy guidelines and should be a national priority. Four specific examples follow.

Coal Gasification - $35 Million

One of the most promising long-term technologies meeting the Administration's guidelines for federal funding of energy R&D is coal gasification. While the technology base underlying surface coal gasification of western noncaking coals is quite mature, additional research is needed in materials, catalysts, instrumentation, and downstream processing. A significant improvement in the economics of coal gasification could result from improvements in the downstream system since it comprises about one-third of the installed plant investment. New and promising approaches that have the potential to lower product costs by at least 20 percent compared to current technology must be developed through the "proof-of-concept" stage. Emphasis should be given to entrained and fluidized bed reactor systems.

Underground Coal Gasification - $10 Million

Underground coal gasification (UCG) offers the largest potential for major reductions in capital investment in the different processes and methods for producing a medium-Btu gas or synthesis gas from coal, but the high risks associated with the requisite oxygen-blown UCG technology currently preclude industry from developing the technology on its own. Before industry will be willing to make major investments, several basic technology issues must be resolved, such as the ability to successfully link modules. DOE should carry this program forward to the "proof of concept" point through multi-modules experiments. The benefits of a successful research program have national policy implications since the government owns or controls over four-fifths of the coal west of the Mississippi. If successful, this technology could significantly increase the value of this land for coal leases and greatly expand the use of the nation's vast coal resources.

Unconventional Natural Gas - $16 Million

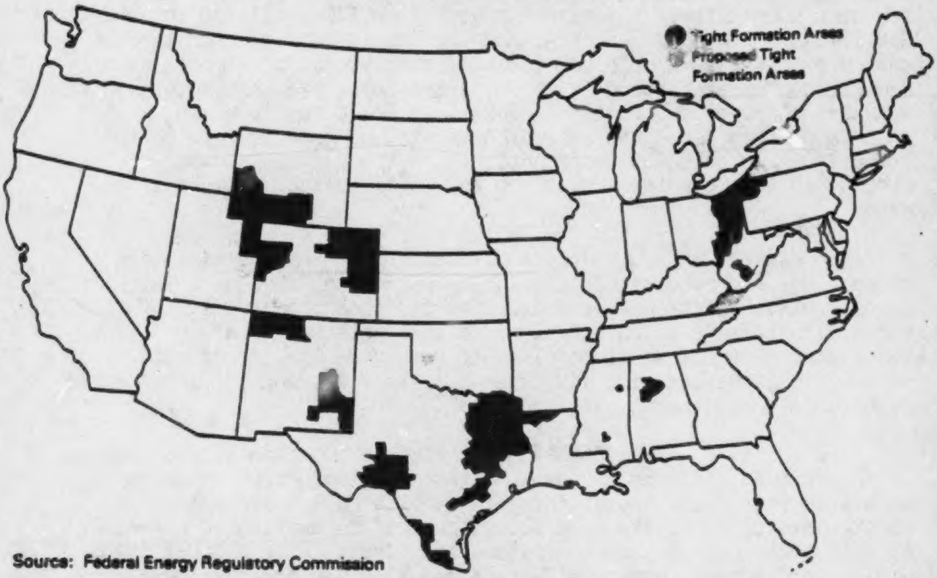
Another appropriate candidate for continued federal support is unconventional natural gas research in the tight lenticular formations in the West and in the Devonian shales in the East. The responsibility for production-oriented research and trial and error drilling should indeed be left with industry. GRI is increasing its research budget for this near-term work from $10.8 million in 1982 to $18.1 million in 1983. Federal funding should support experiments that are unlikely to be conducted by industry, but that could greatly enhance our understanding of these formations. Two particularly good examples of novel experiments unlikely to be conducted by an individual firm since the only product will be generic data on the formations and stimulation technology are the Eastern Mineback Program in the Devonian shales and the Multi-well Experiment in the West.

The Eastern Mineback Program is designed to substantiate the methodology and results of stimulation technology through a series of experiments. Unlike previous experiments, researchers will have the capability to make actual physical measurements and detailed observations of the shale in place to determine the effect of stimulation, the behavior and extent of fracture, and the accuracy of diagnostic tools, among other factors.

The Multi-well Experiment in the tight sand formations located in the West is designed to obtain the maximum amount of technical understanding of lenticular formations. The normal economic constraints of a private firm would prohibit conducting an experiment of this type where wells are drilled far closer to each other than would ever be done commercially to allow flow and pressure testing between wells, well-to-well geophysical testing, and examination of geological continuity. The results are data and information that can be applied by a broad spectrum of industry and are, therefore, of national benefit.

When you consider the National Petroleum Council's (NPC) prediction that development of advanced technology could result in an increase of approximately 200 Tcf of recoverable gas from these formations, then you can understand the benefit to the nation of continuing this long-term research. To illustrate the potential of this technology, the EIA reported that wells currently qualifying under Section 107 of the NGPA as Devonian shale wells have an average production of about 20 MMcf per year or only 55,000 cf per day. At the same time, the EIA reported that Section 107 tight sand formation wells averaged gas production of 60 MMcf per year or 164,000 cf per day. Since Devonian shale formations underlie the Appalachian region and tight sand formations underlie Ohio, Texas, and the Rocky Mountain states (see map below), the successful development of fracturing and stimulation technology to double the production per well would have a tremendous national benefit. This requires a continued federal program in unconventional natural gas research.

Tight Gas Formation Well Areas



Source: Federal Energy Regulatory Commission

Advanced Fuel Cell Technology Development - $15 Million

The gas industry and GRI will propose to make a major increase in funding for onsite fuel cell systems in 1983 (from $11.3 million in 1982 to $19.6 million in 1983 to offset the loss of DOE funds. These funds will be used to continue the manufacturing and field testing of 45 units under the joint program with DOE. This total assumption by industry of funding for the field tests in 1983 and 1984 is consistent with national policy. However, the continued funding of the required technology development program to increase the reliability and lower the costs of these onsite systems cannot be simultaneously absorbed by industry. DOE has a long-term role to develop better materials, catalysts, and heat recovery systems needed to assure that these total energy systems realize their potential. A minimum DOE program of $5 million is required in FY 1983 for this research.

Also, continued long-term research is needed on advanced fuel cell concepts such as molten carbonate and solid oxides. These systems have the potential to be more efficient and to lower overall costs of energy. It is important that the government continue this long-term fuel cell research while the gas industry assumes funding for the near-term research and field testing of prototype units.

It is especially important for the government to maintain a strong long-term gas-related R&D program during the 1982 to 1986 time period while GRI and the gas industry take the lead in near-term R&D. This requires a federally supported long-term gas research program of approximately $100 million (as detailed in the following chart).

RECOMMENDED FY 1983 FEDERALLY-FUNDED GAS RESEARCH PROGRAMS
($ Millions)

Tight Sands (Lenticular)	$ 6
Devonian Shale (Eastern Mineback)	6
Advanced Unconventional Natural Gas (Gas Hydrates, Abiogenic Methane)	4
Underground Coal Gasification	10
Geopressured Methane	16
Coal Gasification	35
Advanced Fuel Cell Technology Development	15
Biomass Conversion	3
Genetic Engineering and Biomass Growth	2
Advanced Gas Prime Mover	4
Hydrogen	3
TOTAL	$104

It should be noted that this recommendation, while totalling approximately $100 million, is less than the level of funds appropriated by Congress for gas-related research in FY 1982 and is only one-third of the FY 1981 funding level. The recommendation is also consistent with the projected funding for Fossil Energy in FY 1983 contained in the Omnibus Reconciliation Act of 1981 since it reduces the actual level of federal expenditures while it continues critical, ongoing, long-term gas R&D programs.

Summary and Recommendations

An adequate gas technology base is a key factor in preventing permanent displacement of gas with imported oil in the industrial and power plant markets. The proposed rapid cutback of the federal long-term gas-related RD&D budget, therefore, significantly increases the danger of increased oil imports. As the gas industry and GRI increase their funding of near-term gas R&D, especially during the 1982 to 1986 time period, it is extremely critical that ongoing federally supported R&D programs related to long-term gas supply and use be continued, strengthened, and adequately funded. This requires a $104 million DOE FY 1983 budget for long-term gas research.

Mr. FUQUA. Now, we will hear from Mr. George Lawrence.

STATEMENT OF GEORGE LAWRENCE, PRESIDENT, AMERICAN GAS ASSOCIATION

Mr. LAWRENCE. Thank you, Mr. Chairman. I am George Lawrence, President of the American Gas Association. We represent some 300 natural gas distribution and transmission companies in the regulated gas utility industry.

What I would like to do is support very strongly what David Webb has said. The Gas Research Institute is the research arm of our industry. I will try to do it briefly and without being in any way duplicative, but I would just like to hit a few basic points.

No industry is more concerned with inflation control than our regulated industry because, if there is one absolute axiom, it is that our rates do not keep up with high rates of inflation. Therefore, we have supported the inflation control programs of this Congress and this administration and we continue to do that.

In support of a $104 million gas-related fossil research budget that Mr. Webb has referred to, I would point out that that is some 30 percent reduced from fiscal 1982. Therefore, we are talking about paring this request to the bone. It is a monumental effort at fiscal prudence, but the programs which we are talking about here are classically in line with the administration's own goal of long-term, high-risk, high-potential research projects.

Again, as a regulated utility industry, we, even more than many other segments in the private sector, are reluctant to take on this kind of research because, if it is successful, it is flowed through promptly to the consumer—and perhaps that is as it should be—but if it is unsuccessful, we very quickly have to bear the burden.

I would point out that to show you the essence of the almost total scrapping of the gas-related fossil research budget, in fiscal 1981 the gas-related research was about 12 percent of the total fossil budget—equal to nuclear fission in 1981.

Nuclear fission has remained at about that 12 percent level of the total budget, but the gas-related research has been slashed virtually to zero.

We say that a balanced research program is necessary. There is no nuclear panacea. There is no coal panacea. There is no oil panacea, no solar panacea, and there is no gas panacea. We need a balanced research program in all of them.

I think that the ones that we have in mind here, again, so classically fit the administration's criteria of high-potential, and high-risk projects.

Mr. Webb referred to the unconventional sands. The potential there is almost mind boggling.

He mentioned the figures of the National Petroleum Council, but let me just stress them again. The National Petroleum Council said that there can be 574 trillion cubic feet of additional gas made available by advanced technology. Now, that is at least 170 trillion cubic feet more than any optimistic projection of current technology. The 574 trillion cubic feet is three times our current level of proved reserves. The 170 trillion cubic feet which is above anything out of the most optimistic current technology projections is almost equivalent to our total proved reserves.

When we get into the areas of the geopressured methane, the gas hydrates, the abiogenics, we are talking about virtually inexhaustible forms of energy.

Therefore, if lightning can strike and there can be an energy research breakthrough, you are talking about some major long-term energy solutions that are worth the effort.

Mr. Webb mentioned the benefits of coal gasification projects to better use our vast coal supplies in the eastern caking coals. In situ coal gasification opens up a whole new potential that doesn't get into the mining problems.

I think that Dave, again, hit the points in some detail, and I won't take up your time to repeat them. However, I would just urge you to seriously consider this and support us in this $104 million, bare-bones, high-risk, high-potential, tremendously high-potential gas-related research.

Thank you, Mr. Chairman.

[The prepared statement of Mr. Lawrence follows:]

STATEMENT OF
GEORGE H. LAWRENCE, PRESIDENT
AMERICAN GAS ASSOCIATION
BEFORE THE
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
OF THE
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
ON THE FY 1983 DOE BUDGET
MARCH 24, 1982

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Introduction

Mr. Chairman and Members of the Subcommittee:

On behalf of the American Gas Association, which represents nearly 300 natural gas distribution and transmission companies serving over 160 million U.S. consumers in all 50 states, I am pleased to provide you with our industry's views on the Department of Energy's FY 1983 budget.

Natural gas currently provides about 27% of the energy consumed in the U.S. Gas is used in about 55% of all residential and commercial establishments in the U.S., and provides about 40% of the energy consumed by U.S. industry and agriculture. More gas is produced domestically than any other fuel, and gas provides two and one-half times as much end use energy to consumers as electricity.

It is important to understand that no sector of the economy is harder hit by inflation than the utility industry. Therefore, A.G.A. is firmly committed to the budgetary goals expressed by the Administration and by Congress; that is, reducing inflation, revitalizing the economy, and reducing the federal deficit to the extent possible. The proposed cuts in the fossil energy research and budget is not, however, an appropriate means to reach these goals. While we recognize that this is a frequently heard statement on Capitol Hill, we believe there are compelling reasons, consistent with sound national energy policy, why this is so.

Funding Criteria

A.G.A. agrees with the Administration that the federal government should fund only those projects that are high-risk, long-term, and have high potential payoffs. We do not share the Administration's apparent belief that only nuclear programs fit within those criteria.

A.G.A. also agrees with the Administration that the federal government should not fund projects that would otherwise be funded by the private sector. However, that criteria does not justify the near-elimination of the fossil energy research budget. Much fossil energy research -- particularly the gas-related research -- cannot be performed exclusively by the private sector for a number of reasons.

First, many valid high-risk, term projects will not be undertaken by any party -- including any gas utility -- without some government assistance. Advanced gasifiers to convert eastern "caking" coals into gas, new technologies to convert biomass to gas, and all end-use conservation research are projects of little interest to oil and gas producing companies. Those companies are primarily interested in technologies which will increase production --

not reduce consumption or develop substitutes. The long-term research necessary to develop western tight sands and eastern Devonian shale resources fully will not be undertaken by gas producers -- especially in light of the fact that a test well cannot usually be worked-over into a production well. Producing companies, in their legitimate self-interest, will be primarily interested in how much production can be obtained in the near-term using current or near-term technology. More exotic technologies needed to develop abiogenic methane and geopressured methane resources are even less likely to be developed by anyone other than the government.

Second, the gas utility industry lacks the financial resources to conduct long-term, high-risk research. Gas industry funded organizations such as the Gas Research Institute (GRI) and the Institute for Gas Technology (IGT) are increasing their commitment to research as quickly as feasible in order to take responsibility for near-term projects. It is difficult for gas utilities to fund long-range, high-risk research directly. As DOE's Energy Research Advisory Board (ERAB) stated in its Federal Energy R&D Priorities, November 1981, "Both these regulated industries (the gas and electric utility industries) have weak incentives to spend on R&D. If successful, the benefits go to ratepayers; if unsuccessful, the expenditures may be disallowed as 'imprudent'." In addition, many states have not given utilities adequate rates of return in the past. As a result, many gas utilities are in no position to fund long-term, high-risk research projects.

Finally, the gas utility industry will need to dedicate its financial resources to more near-term supply sources and to construction and maintenance of pipelines, mains and auxiliary utility facilities. A recent A.G.A. analysis, "A Forecast of Capital Requirements of the U.S. Gas Utility Industry to the Year 2000: 1982 Update," January 6, 1982 shows that the gas utility industry will require $481.6 billion (1982 dollars) between 1982 and the year 2000. About $130 billion will be used for traditional construction and maintenance activities. The remaining $352 billion will be necessary to develop new gas supplies including Alaskan gas, coal gas, and unconventional gas reservoirs. This requirement is over and above investments likely to be made by traditional oil and gas producers. Obtaining these needed funds will be nearly impossible without government research to prove technological feasibility. There is no "slack" in the system allowing gas utility funding for long-term, high-risk research.

Supply Benefits of Improved Technology

Policy makers have become increasingly aware that the supply outlook for natural gas is very good. In the short term, we have had a situation where artificial demand restraints and NGPA production incentives created a gas supply "bubble" resulting in gas that is available but cannot be sold in some parts of the country. In the long term, the A.G.A. Gas Supply Committee study released in 1980 and updated recently showed the great potential of natural gas to help solve our energy problems.

The A.G.A. Gas Supply Committee, made up of a broad representation of gas industry leaders, undertook a comprehensive assessment of the ourlook for conventional and supplemental sources of gas energy to the year 2000. Table 1, developed by the Committee, shows that, while most gas supply estimates are subject to some variation, the largest areas of uncertainty in supply estimates are for the tight formations, coal gasification and nonconventional categories such as biomass conversion to gas, coal seam methane, and geopressured gas. This uncertainty is due to the greater need for technology advancement if these areas are to achieve their high potential.

TABLE 1

GAS SUPPLY -- 2000 (in Tcf)

Supply Sources	Self Sufficiency	World Conventional Gas Emphasis
Lower 48	13-14	12-14
SNG	0.3	0.1
Alaska	2.8	2.8
Canada	1.0	2.0
Mexico	0.1	2.0
LNG	0.7	2.5
Coal Gas	2.7	1.3-2.5
Tight Formations	1.2-3.0	1.2-3.0
Nonconventional	1.0-2.5	1.0-2.5
Totals	22.8-27.1	24.9-31.4

The key point to be recognized in the above chart is that the difference between high and low technology development of tight formations, coal gas and nonconventional sources is on the order of 5 Tcf. It is also interesting to note that 5 Tcf is equivalent to nearly 2.5 million barrels of oil per day.

Because it is difficult for the gas utility industry to fund long-range, high-risk research for regulatory and fiscal reasons, A.G.A. believes that achievement of the supply goal of 31 Tcf -- and the concommitant benefits -- is conditioned on the government financing a part of the long-term, high-risk research.

The Need for a Balanced Research Program

History is replete with examples of the dangers of relying on a single source for any valuable commodity. At the present time, the U.S. produces natural gas, coal and oil at a reasonably well-balanced level: according to DOE's Monthly Energy Review, February 1982, production in 1981 of dry natural gas was 19.978 quadrillion Btus (quads), coal was 18.253 quads, and oil was 18.125. By contrast, nuclear electric power was only 2.908 quads. It should be noted that our consumption of energy is not so well-balanced: petroleum consumption in 1981 was 32 quads compared with 20 quads of natural gas and 16 quads of coal. Lifting federally-imposed restraints on gas use, promoting coal gasification and natural gas fueled vehicles, and developing better, more efficient end-use equipment could result in a better balance of consumption. Such a balance could eliminate the need for oil imports.

Different analysts have differing views on the fuel supply mix that the U.S. will have by the year 2000 and beyond. One thread in common, however, runs through all analyses: fossil fuels will remain the predominant source of energy. The analysts differ only in their perception of the relative minority shares to be supplied by nuclear, solar, wind, ethanol and other "alternative" supplies.

Given our nation's dependence on fossil energy, now and in the foreseeable future, it is bad public policy to eliminate the fossil energy research program.

Although A.G.A. supports reductions in federal spending, it does not support the drastic and unbalanced reduction in the fossil energy share of the R&D budget. Only the fossil and renewable energy areas have suffered a reduced share of the total R&D budget while over the same period, all other areas have remained constant or increased. (See exhibit attached). Specifically, fossil research has been slashed from 12% of the total in 1981 -- a share roughly equivalent to that of nuclear fission programs in 1981 -- to a clearly insufficient 1% allocation proposed in FY 1983. There are many good gas-related projects that are worthy of an increased share of Federal support.

Recommended Gas-Related Research Projects

A.G.A. recommendations, developed through deliberations by our Technology Committee and in consultation with the Gas Research Institute and the Institute of Gas Technology, reflect what we believe to be the minimum level of funding consistent with the ideal of a balanced energy research program. Funds are recommended only for projects with high potential and commensurate high risks.

A. Unconventional Natural Gas: $32 million

- Estimates of the gas resource trapped in the Western tight sands of the Rockies, Northern Great Plains and Southwest, in lenticular and blanket formations, range from 400 to 1000 Tcf. While 0.5 Tcf/year is currently being produced, substantial new production is dependent on the development of advanced technology. Almost 90 percent of the resource base is in lenticular formations which cannot be produced with current technology. Production estimates in the year 2000, given only existing technology, will probably be less than 3 Tcf/year while development of advanced technologies could increase production to approximately 6 Tcf/year.

- Devonian shales located in the Appalachian, Michigan and Illinois basins contain a DOE estimated 200 to 2000 Tcf of gas in-place. In addition to the 0.1 Tcf/year produced commercially from Devonian shale today, approximately 200 new wells were drilled in 1980. Based on existing technology only, the estimated production from Devonian shale formations would be less than 0.8 Tcf/year by the year 2000. This production level could double through the development of advanced technologies.

- Geopressured aquifers[1] of the Gulf Coast may contain between 42 and 2000 Tcf of recoverable gas dissolved in saturated brine. Significant uncertainties must be addressed in a well-conceived and coordinated research and development program to examine type and size of the resource, long-term production rates, impact of off-set wells, cost of gas production and environmental impacts. Until these and other questions are answered through an expanded research and development program, it is impossible to properly ascertain the long-range energy contribution of geopressured aquifers.

- Advanced unconventionals such as gas hydrates and abiogenic methane truly represent a high potential payoff. Gas hydrates are an ice-like mixture of gas and water in which gas molecules are trapped by a framework or "cage" of water molecules. Abiogenic methane is gas that is not biologic in origin. Some scientists believe vast quantities of such gas could be found in extremely deep reservoirs.

  At the present time, the resource base of gas within these reservoirs is poorly defined and theoretical. The amount of gas which is recoverable is even more speculative. No one is predicting a near-term breakthrough, but some observers believe that these potential sources of gas could some day produce virtually unlimited amounts of energy. Government support is needed to increase knowledge of the resource base and reservoir mechanics for these advanced unconventional sources. Only with more knowledge will we gain an understanding of costs and potential benefits of developing these sources.

- In-situ coal gasification has the potential to significantly expand economic attractiveness of coal seams which are currently unmineable. A variety of technical questions must be answered before significant amounts of gas can be produced through in-situ gasification processes.

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[1] The geopressured methane program's budget is not subject to this Subcommittee's jurisdiction.

In order to convert these tremendous resource bases into viable producing reservoirs, advanced technologies need to be developed.

Ongoing DOE research in tight sands is focused on the Multi-well drilling program in Garfield County, Colorado. The proposed budget, intended to bring this experiment "to a successful conclusion,"[2] will instead end the experiment prematurely. Three test wells, as originally planned, would provide a three-dimensional picture of the extent to which fracturing occurs throughout multi-lens geophysical areas, thereby boosting the prospects for developing tight sands reservoirs. Approximately $11 million has been spent so far in drilling the first two of these wells.

It is important to recognize that the private sector is doing considerable research in the area of tight blanket sands. The Multi-well Experiment, however, is examining tight lenticular sands -- formations shaped somewhat like lenses where the pay zone may extend a thousand feet measured vertically, but, horizontally, is not as large as conventional or blanket sands. The private sector has the means to perform the near-term research necessary to develop blanket sands, but the long-range, high-risk technology necessary to develop lenticular tight sand reservoirs -- that contain 90 percent of the resource -- must be government-sponsored.

Such sponsorship is justified. The National Petroleum Council (NPC) estimates that with advanced technology tight sands may hold 924 Tcf of recoverable reserves -- 170 Tcf more than could be recovered under NPC's most optimistic current technology scenario and nearly three times our current level of reserves.

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[2] Congressional Budget Request FY1983, U.S. Department of Commerce, Energy Research and Technology Administration, Vol. 5, pg. 113.

The proposed budget also eliminates the Devonian shale program. DOE estimates that as much as 2,000 Tcf of gas may be located in the shales of the Appalachian basin. Funds expended up to this point have been used to develop the resource base estimate. Now that we have a reasonably good understanding of how large the resource base is, it is foolish to eliminate the program until we have a better understanding of how to produce that gas. Estimates made by the A.G.A. Gas Supply Committee -- in conjunction with GRI -- indicate that the level of recoverable Devonian shale gas is at least as dependent on advanced technology as it is on price. Judiciously funded research would provide a doubling in recoverable resources without substantial price impacts. The Eastern Mineback experiment should be funded so as to increase our knowledge of fracturing shale and producing gas.

The bulk of the funds needed to see these two vital experiments to completion would be expended over the next few years to drill or mine at the site. Relatively small levels of funding will be needed after that in order to test various fracturing and other production enhancement techniques.

A.G.A. also recommends that continued research be performed to research and develop methods for production of abiogenic methane, methane from coal seams, and gas hydrate resources. The existence and location of the abiogenic methane resource must be confirmed in order to determine a method of recovery. Extraction and production methods for coal seam methane based on increasing the permeability of the coal must be established. Fundamental laboratory work on gas hydrate formations should begin in FY 1983. Funding levels in these areas should be sufficient to ensure that ongoing programs maintain continuity. Only with more knowledge will we gain an understanding of costs and potential benefits of developing these sources.

B. Coal Gasification : $45 million

The production of synthetic fuels will be a major contributor to the long term energy supply. By the year 2000, synthetic fuels are forecasted to supply 3 to 5 million BOE/day. Coal gasification and indirect coal liquefaction will be the major synthetic fuels, accounting for over half of this total. Nuclear energy, by contrast, will account for about 3 to 4 million BOE/day by the 2000 in spite of its current limited role (1.4 million BOE/day) and massive federal support.

The United States is estimated to have coal reserves of some 438 billion tons, containing 10,000 quads, one-half of which can be recovered under present technical and economic conditions. This abundant domestic resource can be converted to environmentally benign methane which can be used to replace imported oil in virtually every industrial stationary use.

While first-generation surface coal gasification is a proven technology, advanced gasifiers that can use agglomerating high-ash coals require further development. Such technology would allow gasification plants to be sited in areas hard-hit by the current recession such as: Illinois, Kentucky, Ohio, Pennsylvania, Tennessee, and West Virginia. Because direct combustion of much of this eastern coal is environmentally prohibited, gasification is the best way to encourage production, spur job creation, and provide these states with a usable domestic fuel. A.G.A. projects that as much as 2.7 quads could be produced by surface coal gasification by the year 2000.

About 220 billion tons of coal is mineable under present technical and economic conditions. An estimated 3,800 billion tons is not currently mineable. A substantial amount of this resource base could be suitable for in-situ gasification. DOE research on steeply-dipping beds has attracted wide interest within both the coal and the gas industry. Successful DOE research to improve reliability and efficiency of the process could potentially "quadruple U.S. coal reserves while providing fuels at potentially lower costs and with less adverse environmental impacts than other synthetic fuel options."[3] Despite this glowing tribute to the potential of in-situ coal gasification, the proposed budget recommends termination of all activities except for legally required environmental monitoring.

C. Other Technologies: $27 million

A.G.A. believes that fuel cells and advanced gas prime movers, including heat pumps, offer significant advantages for energy conservation. Residential, commercial, and industrial customers have made great progress in conserving energy, but much of the "easy" conservation such as insulation and thermostat adjustments has already been made. The next step must be development of more efficient equipment. Development of gas-fired fuel cell technology offers industrial and large commercial customers a significant savings in generating heat and on-site electricity. Advanced prime movers that can provide both heat and air-conditioning offer hope to the residential customer -- currently facing increasing electric and gas fuel bills.

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[3] Ibid, page 62.

A.G.A. also believes that biomass may be an important supplemental source of methane in the future. A balanced energy program should fund biomass-to-gas programs at a level sufficient to keep research progressing. To the extent that there is a research breakthrough allowing economic conversion of biomass to gas, this represents an inexhaustible, renewable supply source.

Conclusion

It is difficult for gas utilities to fund high-risk, long-term research projects directly. A pattern of inadequate regulatory decisions has begun to impair the financial standing of gas utilities. Over the next twenty years, the gas industry will have to raise approximately $480 billion (in 1982 dollars) to finance gas supply, transportation, and conservation projects. These funds will be spent primarily on construction to enable the gas utility industry to meet demand. Research and development activities are, however, an integral part of the full development of domestic gas resources and these activities should be strongly supported by Congress, DOE, and industry.

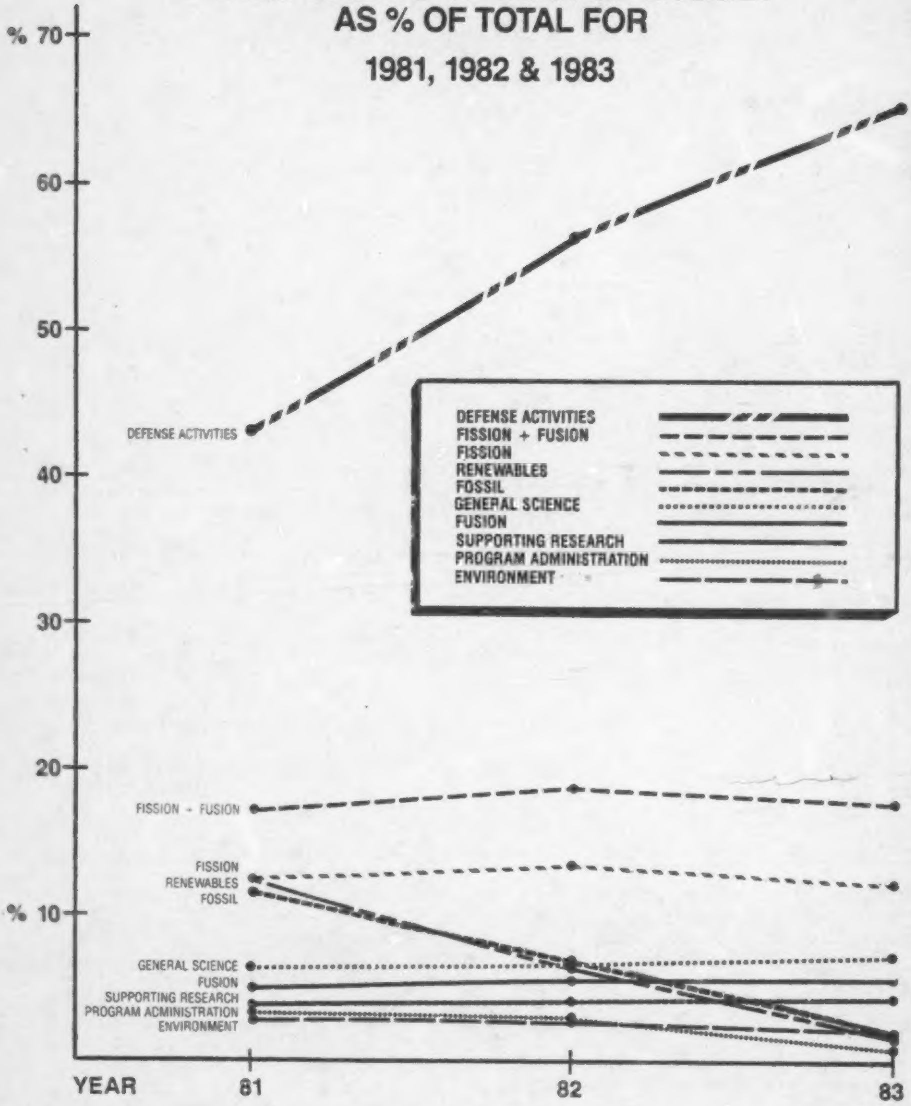
Even though gas provides 2 1/2 times the energy to end users as electricity, the proposed FY 1983 budget slashes gas research programs. Of even greater importance, fossil energy will continue to play the major role in our nation's energy supply, yet the proposed budget reduces fossil energy research to 1% of the DOE budget. A balanced energy research program to allow production and use of fossil energy in an efficient and environmentally sound manner is, therefore, in the national interest.

Gas related programs within such a program should be funded at least at a minimum of $104 million. Research priorities should include: $32 million for unconventional natural gas reservoirs such as tight sands, Devonian shale, hydrates, abiogenic methane, and geopressured zones; $45 million for surface and in-situ coal gasification; and $27 million for fuel cells, advanced gas prime movers, and biomass conversion to gas.

The key concept behind the need for R&D in all these areas is that we cannot mortgage the next 20 or 30 years while waiting for technology to develop in areas such as nuclear fusion. The price incentives currently in place are sufficiently high enough to allow exploitation of conventional and unconventional resources with current technology. It has been shown, however, that the commercial recovery of these resources are as dependent on improved technology as on price. It would be short-sighted to put undue emphasis on price incentives and ignore the potential of improved technology to allow greater recovery of these resources.

Gas is an ideal bridge fuel that will enable us to enter the 21st century with adequate domestic energy sources. The ability of the gas industry to meet the nation's energy needs in the 21st century will depend substantially, however, on whether R&D work is begun now.

COMPONENTS OF DOE R&D BUDGET AS % OF TOTAL FOR 1981, 1982 & 1983



Mr. **Fuqua**. Thank you, Mr. Lawrence. Now, we are very pleased to hear from Dr. Bernard Lee who has been here many times, also.

Dr. Lee, we are pleased to have you.

STATEMENT OF DR. BERNARD S. LEE, PRESIDENT, INSTITUTE OF GAS TECHNOLOGY

Dr. **Lee**. Thank you, Mr. Chairman. I am glad to be here. I am Bernard Lee, President of the Institute of Gas Technology, a not-for-profit organization in Chicago which has specialized in energy R. & D. and energy education for the last 40 years.

In developing energy technologies, our concern is for the preservation of the continuity of effort from laboratory-scale studies through commercial-scale demonstration to fully establish commercial and technical feasibility.

While we strongly support participation by the private sector in the development of new energy technology, we must be realistic about the need for Government support where the cost to get to commercial demonstration, even with tax incentives, loan guarantees, and price guarantees, is so high that few companies can support such effort.

We are also concerned that diminished Government support for energy research and development—especially development—for even a few years will cause an energy-technology gap by the dissolving of existing centers of technical capability and dispersal of highly qualified personnel. An energy-technology gap of at least a decade could result from the current direction of the Government's energy program.

The new technology necessary to meet future energy needs cannot be developed by turning energy R. & D. on and on like a faucet in response to short-term fluctuations in the world energy supplies and prices over which we have little control.

Although over the last [illegible] years the energy picture in the United States began to look a little brighter, this was due to the economic slowdown, the continuous substitution of domestically produced energy for imported oil, and conservation due to the improvements in the efficiency of energy use.

However, it will be foolish to allow these positive trends to lull us into complacency. First, the demand reduction will not continue at the same rapid pace because the more easily accomplished conservation measures have already been taken.

Second, although many users were able to substitute coal for oil, the next increment of coal substitution will be more difficult and costly.

Most importantly, however, any economic upturn, which is anticipated sometime this year, will immediately trigger increased energy demand.

On the supply side, despite the recent heightened activity in oil and gas production, most of the drilling in the United States has been in increased production through established reservoir drilling rather than drilling for new exploration.

The stability of the present oil market is so fragile that it could be shattered overnight by any upheaval in the Middle East. The continuing war between Iran and Iraq, the export of the Khomeini brand of Islam, the menacing Soviet presence in the Persian Gulf, each is a short fuse that could set off major oil disruption.

The current so-called oil glut should be regarded as a temporary respite, a breathing spell which gives us time to develop alternatives for the day when oil imports will not be available under acceptable conditions.

It took an oil surplus of only 2 million barrels a day, less than one and a half percent of the world's annual energy consumption, to transform the world oil market from a seller's to a buyer's market in 1981, and the reverse could easily happen.

We must recognize that we and our allies are already in a severe recession, and another upset in the world's oil supplies could be much more crippling than the upsets of 1973 and 1978.

I, as an individual, have always advocated letting free-market forces work; however, our free-market economy has never been totally free. We have a long tradition of peacetime public investment in those areas vital to our well-being, national security, and prestige. The National Highway System, support for the farm industry, space programs, and defense are national decisions that transcend the free market, and must be undertaken by society as a whole.

Energy is so vital and so central to the Soviet-U.S. power balance and the economic stability of the world that the Congress and the administration must place the same priority on energy that they do on defense.

Therefore, insuring a commercial-scale capability for an adequate and secure energy supply outweighs all other considerations. National policy must focus on energy because the private sector could never factor national needs into investment decisions.

We at IGT fully support the $104 million minimum gas-related R. & D. budget for DOE in fiscal year 1983 which was recommended earlier by Mr. Webb and Mr. Lawrence.

As a performer of energy R. & D., I will take a minute to address a number of specific areas in energy supply, energy conservation, and solar R. & D. in which IGT is actively engaged.

In unconventional gas, IGT has developed an accurate method of measuring the natural gas produced with geopressurized well water. Sixteen million dollars is needed to obtain additional basic data to access the ability of geopressurized aquifers to produce gas in excess of solubility in reservoir brine. These tests will be conducted on wells originally drilled in the search of geothermal energy.

IGT has established the definitive DOE standard method of determining the permeability of tight sand formations and how the reservoir volume changes as gas is produced. Six million dollars is needed to obtain additional tight sand core samples for laboratory study.

Four million dollars is needed on research on advanced unconventional natural gas resources including gas hydrates, abiogenic methane, and coal seam natural gas.

In the area of coal gasification, which as you know, IGT has had a long history of R. & D., we recommend that $4 million be authorized to continue basic support studies at IGT for the improved catalysts, understanding of ash behavior, analysis of fluidized bed systems, and improved waste processing and product upgrading.

One million dollars is needed to continue the Coal Conversion System Technical Data Book by compiling, evaluating, and correlating the necessary design data for coal conversion and utilization.

As a pioneer in developing processes to utilize our Nation's large peat resources, we have successfully completed pilot plant testing of the peatgas process to produce SNG from peat.

The high-pressure lock-hopper system for which this subcommittee authorized funds last year was installed and successfully operated.

For fiscal year 1983, we recommend $3 million to continue work on peat harvesting, dewatering by wet carbonization, resource and environmental assessment.

We recommend $7 million for the development of the pressurized ash-agglomerating gasifier which is applicable to hydrogen production, combined-cycle powerplants, and methanol synthesis.

In the fuel cell areas, IGT is a leader in molten carbonate fuel cell technology. It recommends $7 million to continue the second-generation fuel cell development, including $3 million for fundamental research to determine performance and life characteristics for fuel cell materials as well as multifuel capability research. Molten carbonate fuel cell offers high efficiency in power generation, plus economic cogeneration of high-pressure steam.

For third generation, solid oxide electrolyte technology development, IGT requests $3 million for laboratory testing to solve high-temperature materials problems.

Going to the biomass area, we recommend $1 million to operate the process development unit of the Renugas process to convert forest and biomass materials to fuel. The construction of this PDU will be completed in fiscal year 1982.

The sum of $2 million is recommended for growth and harvesting development and basic genetic engineering research in biomass production.

Now, hydrogen is a long-term fuel that can be produced with baseload power stations, stored, and later utilized either as a clean fuel or for power generation. Also, $3 million is needed for a sustained effort in thermochemical conversion of solar energy to hydrogen, and in the fundamental studies of catalytic combustion of hydrogen.

Finally, in addition to the above, for onsite commercial or industrial usage of solar energy, thermal energy storage through the use of molten salts can be applied to processes that require high levels of periodic thermal energy demand such as the high-temperature furnaces and kilns that are common to the brick, clay, ceramic, and cement industries. And $8½ million is needed to continue the molten salt storage R. & D. to address the applications, operating requirements, and storage media configurations.

Mr. Chairman, my list of specific funding needs may seem long, but it merely reflects the holes in the administration's proposed program in nonnuclear energy R. & D. A sustained research and development program will assure a continuing flow of improvements and innovations. Coupled with a growing synfuels industry, it will be the most effective insurance the Nation can provide against oil-related economic disruption.

I thank you for this opportunity to present my views.

[The prepared statement of Dr. Lee follows:]

Statement by Bernard S. Lee, President
Institute of Gas Technology
Before the House Committee on Science and Technology
Subcommittee on Energy Development and Applications
Hearings on Department of Energy Budget Authorizations

March 24, 1982

Mr. Chairman and Members of the Subcommittee:

My name is Bernard S. Lee. I am President of the Institute of Gas Technology, a not-for-profit energy R&D organization affiliated with Illinois Institute of Technology in Chicago. IGT has specialized in energy RD&D work and energy-related education programs for 40 years. I will provide you today with my assessment of the continuing energy RD&D needs of the nation.

In past years IGT appeared before Congress to offer progress reports on various research projects in which we were engaged. During the previous Administration our work on synthetic fuels technology received much encouragement. Congress strongly supported this energy technology development in response to the oil supply disruptions and price spikes in the 1970's.

Now, however, government energy policy is directed toward funding long-term high-risk research, with the expectation that private industry will start funding much energy RD&D that was formerly carried out by the Department of Energy. Government involvement will focus on the areas of basic science and applied research, leading toward, and perhaps including proof-of-concept. Process development and demonstration, which are the necessary precursors of commercialization, would be left to the private sector.

Our concern is for the preservation of the continuity of effort from laboratory or bench-scale through the larger process development units, pilot plants and then to the size required to assess commercial and technological feasibility. While I strongly support participation by private enterprise to the maximum extent possible in the development of new energy technologies, I believe we must be realistic about the need for government support of high-risk new technologies where the cost to get to commercial demonstration (even with tax incentives, loan guarantees, and price guarantees) is so high that few, if any, major companies can support such efforts.

We are also concerned that with diminished government support for energy research and development, especially development, an "energy technology gap" will be created. Reduced support of energy technology development for even a few years will surely result in the dissolution of centers of technical capability and the dispersal of highly qualified technical personnel to other endeavors. In that case, an entire new generation of technical and scientific personnel will have to be trained again and given sufficient practical experience before it can contribute to energy technology development programs. In short, an "energy technology gap" of at least a decade could result from the current, and in my view, short-sighted, thrust of the government's energy programs. The new technology necessary to meet future energy needs cannot be provided by turning energy RD&D on and off like a faucet in response to short-term fluctuations in world energy supplies and prices over which we have little, if any, control.

I have always advocated letting free-market forces work. However, our free-market economy has never been totally free. We have a long tradition of peace-time public investment in those areas vital to our well-being, security and prestige as a nation. The national highway system, support for the farm industry, space programs and defense are national decisions which transcend the free market and must be undertaken by society as a whole.

Energy is so vital and so central to the power balance between the U.S. and U.S.S.R. and the economic stability of the world, that the Administration must place the same priority on energy as it does on defense.

Although for the past two years the energy picture began to look brighter, this was due to the economic slow-down, the continued substitution of domestically produced energy for imported oil, and conservation due to the improvements in the efficiency of energy use. However, it would be foolish to allow these positive trends to lull us into complacency. First, demand reduction will not continue at the same rapid pace, because the more easily accomplished conservation measures have already been implemented. Further and sustained improvements will become more difficult. Second, although many users were able to substitute coal for oil, the next increment of coal substitution for imported oil will be more difficult and costly. Most importantly, any economic upturn, which is anticipated sometime this year, will immediately trigger increased energy demand.

On the supply side, despite recent heightened activity in oil and gas production, most of the drilling in the U.S. has been to increase production of established reservoirs rather than for new exploration. Although the decline in our oil reserves has leveled off, the stabilization of reserves estimates is due largely to a revision of estimates of previously discovered reserves. New discoveries were equivalent to only 29% of production in 1980. Although, undoubtedly, we do have substantial amounts of oil yet to be found, it is generally accepted that the potential for large-scale new discoveries is limited.

The stability of the present world oil market is itself a fragile thing that could be shattered overnight by any upheaval in the Middle East. Iran remains in the throes of political change and is still at war with Iraq. Further, one of the main motivations for OPEC's moderate attitude toward price is fear -- fear that the West will finally develop alternative energy sources that could, if not put it out of business, at least act as a constraint on its control of pricing. Sheik Yamani, the Saudi oil minister, is on record saying that "If we force the West to invest heavily in finding alternative sources of energy, they will. This would take no more than seven years and would result in reducing (The West's) dependence on oil as a source of energy to a point that would jeopardize Saudi Arabia's interest."

The current world oil "glut" should be regarded as a temporary respite, a breathing spell that gives us time to develop alternatives for the day when imported oil will not be available under acceptable conditions. We should keep in mind that it took an oil surplus of only 2 million barrels a day, less than 1.5% of the world's annual energy consumption, to transform the world oil market from a seller's to a buyer's market in 1981. The reverse could easily happen. The question that must be answered is how can we maintain secure and dependable energy supplies, both as an individual nation and as the leader of the free world. We must also recognize that we and our allies are on the downward slope of a rather severe recession, and another major upset in the world's oil supplies could have a much more severe impact than the upsets of 1973 and 1978.

Therefore, insuring a commercial-scale capability for an adequate and secure energy supply outweighs all other considerations. National policy must establish energy as a high priority because the private sector can never factor national needs into investment decisions.

We fully support the $104 million minimum long-term gas-related federal R&D Budget for FY '83 recommended by the gas industry. The following are specific areas of R&D in which IGT is actively engaged.

Unconventional Gas

During the past year IGT continued research on unconventional gas including field projects, laboratory research and analytical studies. The principal areas of work were determination of geopressured/geothermal aquifer gas content and production prospects, laboratory characterization of reservoir rock and stimulation procedures for gas production from tight sands and coal seams.

o IGT developed and utilized an accurate method of measuring natural gas produced with geopressured well water. Use of this method demonstrated higher gas production than anticipated in previous theoretical gas production studies. In one case an abandoned well, which had been judged a dry hole from conventional test procedures, produced so much natural gas that it is still in commercial production 17 months later.

In FY'83, $16 million will be required to obtain additional basic technical information needed to assess the portion of geopressured aquifers capable of producing otherwise undiscoverable gas in excess of solubility in reservoir brine. Tests would be conducted on wells originally drilled in search of geothermal energy. Test equipment already in place at the geothermal well sites would be augmented with the unique instrumentation and analytical procedures developed by IGT.

o IGT has established the definitive DOE standard method of determining the permeability of tight sand formations and how the reservoir volume of tight formations changes as gas is withdrawn during production. Continuing basic work is needed on means to answer such questions as gas content of the sand adjacent to the well bore, how the gas flows through the sand and the size of the lens or reservoir. It is anticipated that the answers to these questions will increase the recoverable resource by 70-90 trillion cubic feet. We recommend a $6 million funding level to permit tight sand core sample removal and laboratory study. Coordination between laboratory operations, well drilling and well testing will be essential.

o $4 million is needed to permit further research on advanced unconventional natural gas resources including gas hydrates, abiogenic methane and coal seam natural gas. Gas hydrates are solid compounds of methane and water which form under conditions of low temperature and high pressure. Gas hydrates have been found in arctic regions of the world and enormous quantities are believed to be present in sediments in the deep ocean bottom. Abiogenic methane is natural gas from non-biological origins. It has been hypothesized that much abiogenic gas was trapped in the earth when it condensed from the nebula surrounding the sun billions of years ago. Both gas hydrates and abiogenic methane are very long-term, high-risk, possible future sources of gas; but we need to know more about the trapping and transport characteristics in source rock in order to evaluate methods of recovery.

IGT's research on gas production from coal seams has revealed that conventional well completion procedures, such as hydraulic fracturing, are much less effective than in conventional gas reservoirs due to fundamental differences between coal and the sandstone or limestone rock in conventional reservoirs. As with hydrates and abiogenic gas, fundamental research on gas trapping and transport in coal is needed for devising more effective well completion procedures.

Coal Gasification

As you know, IGT has a long history of research and development in coal gasification. We recommend the following actions, consistent with the Administration's policy of long-range research to improve the efficiency of coal gasification processes.

- o Continuation of essential work by IGT is recommended in the development of improved catalysts to enhance the efficiency of coal gasification processes, studies of coal ash behavior to improve coal utilization and gasifier operation, studies of fluidized-bed systems for improving coal gasification processes and investigation of improved processing techniques for product and waste streams. These basic research and support studies would require $4 million in FY'83.

- o Since 1974, IGT has been compiling, evaluating and correlating the necessary design data to be applied to numerous coal conversion and utilization projects -- the Coal Conversion Systems Technical Data Book. These data are presented in forms needed by engineers and scientists engaged in projects or developments within the emerging synthetic fuels industry. As such, the Data Book helps foster the transfer of government-supported technology development to the point of commercial application by private industry. $1 million to continue the Data Book effort is necessary to enhance the ability of industry to undertake major gasification projects.

- o As a pioneer in developing processes to utilize our nation's large peat resources, the peat gasification program at IGT achieved major milestones during the past year. The HYGAS plant has been successfully modified for operation on peat; high-pressure lockhopper operation for feeding of peat has been demonstrated; carbon conversions in the PEATGAS process have exceeded 90% with high yields of methane.

  $3 million is needed to continue work on peat dewatering, harvesting, resource and environmental assessment. Further development of wet carbonization of peat is also included to provide suitable feedstocks for either gasification, liquefaction or direct combustion of peat.

- o $7 million is needed for the development of the pressurized ash-agglomerating gasifier which could have broad application in such diverse areas as industrial fuel gas production, hydrogen generation for chemical production, combined cycle power plants and methanol synthesis. The ash-agglomerating process, with its superior method of feeding and ash discharge, results in better separation of ash residue, improved carbon conversion (efficiency) and produces a non-leachable ash. These efficiency and environmental considerations are clearly worthy of further experimental development. Of the total, $3 million is for modifying an existing facility and $4 million for operation and preliminary testing.

Fuel Cells

Phosphoric acid fuel cell technology has received sustained government funding to permit it to reach the demonstration phase. It is now important for continued government funding at an accelerated pace for development of the molten carbonate system because of its superior performance and energy conservation potential.

The molten-carbonate fuel cell offers the utility industry a means to generate power from coal at approximately 50% cycle efficiency which is a significant improvement over a conventional steam plant. IGT has invested nearly 500 man years of professional expertise in fuel cell R&D in a twenty year period, and maintains extensive laboratory and development facilities for fuel cells and related fields. As a result, IGT is unique in the U.S. and probably in the world in its combined knowledge and expertise in the areas of fuel cells, coal gasifiers, and gas cleanup systems. Molten carbonate cell technology has been proven by IGT at the single cell level in cell sizes up to one square foot. Some testing of multiple cell stacks containing as many as 20 cells of one square foot area has begun. Future RD&D needs include scaling cell sizes up to the commercial level, further testing and verification of gas manifolding, temperature control and cell configuration, and demonstration of the technology using 5 to 200 cells of the one square foot size. Additional molten carbonate R&D is required to develop improved fuel cell components and fabricate them into prototype stacks to verify system efficiency and production costs.

Japan, as part of its Project Moonlight, is focusing on all facets of fuel cell technology and is committing 11 billion yen (U.S. $50 million) over the next six years. In the U.S., EPRI and GRI are committing their financial resources for fuel cell development primarily in support of near-term (phosphoric acid fuel cell) commercialization. There is no existing fuel cell industry to take on the developmental responsibility and associated risk of the longer-term technology. Therefore, it is essential for the government to provide support until the technology and the industry are both ready for commercialization.

- $7 million is required to continue molten carbonate fuel cell (MCFC) technology development. Of this amount, $3 million should be allocated for fundamental MCFC technology research needed to determine performance and life characteristics for candidate fuel cell materials as well as multifuel capability research. The direct feeding of gaseous or liquid hydrocarbon to the MCFC, using internal reforming, has been tested at IGT and the very promising results indicate such a system could be substantially simpler, with increased power generation efficiency, reduced cost and improved reliability. These R&D activities are necessary for the earliest application of MCFC technology in the industrial and commercial sectors. In order to disseminate information on cell life and performance directly to all potential manufacturers, the use of DOE's existing endurance prototype testing facility and controlled environment facility would be required.

- $3 million is necessary to continue a broad program in third generation solid oxide electrolyte technology. Extensive laboratory testing is necessary to meet the challenging materials problems inherent in a system operating at 1800°F. Laboratory work is needed to find candidate materials to handle the higher temperatures and to determine the life of cell components. These efforts would begin to lower material costs.

Biomass

- With DOE funding, work on IGT RENUGAS process (steam-oxygen gasification of forest and plant biomass materials for fuel) has proceeded from laboratory and bench scale studies to the design and construction of a process development unit (PDU). $1 million is needed to support the operational phase which will provide better understanding of the relationship of biomass feed characteristics to fuel yields and provide the basis for long-term genetic development of feed stock species.

- The production of renewable energy from marine biomass is definitely a long-range supply option. While commercial production of kelp and seaweed is already underway in Japan and China, the methods are very labor-intensive. A more technological approach to growth and harvesting would reduce costs and provide a suitable biomass feedstock for applications in the U.S. Marine biomass production is attractive because it can be done over a much wider area than for land-based biomass, and because marine biomass does not contain lignin and is readily biodegradable.

Digestion of biomass for energy production takes place in three essential steps: (l) hydrolysis and liquefaction, (2) fermentation to produce hydrogen, carbon dioxide and acetic acid, and (3) methanogenesis — the production of methane from the fermentation products. Through isolation and characterization of the key bacterial populations involved, it may be possible to develop special bacterial inocula to enhance and optimize reaction rates for each step, and to isolate the principal and most effective organisms.

$2 million in FY'83 would permit continuation of preliminary work to develop a technological approach to growth and harvesting as well as basic genetic engineering research.

Hydrogen

Nuclear heat or electricity, solar or wind energy and other renewable energy sources are not storable like fossil fuels, and long-distance delivery of most renewable energy forms is impractical without conversion to a chemical fuel form. A long-term solution to those problems is hydrogen. It can be produced with base-load (fossil, nuclear and solar) power stations, stored, and later utilized in both conventional (gas turbine) and advanced (fuel cell) electric power peaking generation equipment. Unlike batteries and mechanical storage devices such as flywheels, hydrogen can be stored for long durations without degradation of energy quality.

o However, before hydrogen can be economical as a fuel in the long-term, much advanced technology development is needed. IGT has been a leader in hydrogen research and recommends sustained support in:

—experimental work on thermochemical conversion of solar energy to hydrogen.

—fundamental studies of catalytic (flameless) combustion of hydrogen.

This program should be sustained over at least a five-year period. A first year funding level of $3 million is essential to begin this work.

Storage

In addition to the above, there is a strong need for energy storage R&D to meet large seasonal changes in energy demand. In order to meet the seasonal heating and cooling requirements in the U.S., 18 times more energy is needed in the winter than in the summer. In the Northeastern U.S. this factor increases to 90 times.

One thermochemical method of thermal energy storage (TES) for on-site commercial or industrial usage of heat energy is the use of melted salt or salt mixtures. Work began in March of 1981 at IGT on a three-year project under DOE sponsorship to develop an advanced energy storage system using molten carbonate salts, and applicable to all industrial processes that require high levels of periodic thermal energy demand. Examples are the high temperature batch-operated furnaces and kilns common to the brick, clay, ceramic and cement industries, although the work is not limited to these specific industrial areas. The feasibility of retaining molten carbonate salts in porous oxide ceramics for thermal energy storage has been proven using low-cost materials which exhibit good stability.

A broad-based generic R&D program is required to address the many applications and operating requirements as well as the many possible storage media configurations identified or thought to be possible. After sufficient research and technology development, industrial trade associations, TES user groups with comparable requirements and potential TES system manufacturers could be involved in transferring the technology to the commercial sector.

o The funding requirement in FY'83 for thermal energy storage RD&D based on molten salts is $8.5 million and will permit the following work:

- Measurement of physical and chemical properies of potential media materials.
- Optimization of media thermal and physical behaviors within expected operating conditions.
- Media selection/development to fit application temperatures and operating requirements.
- Materials compatibility studies addressing media, containment, working fluids and auxiliary components.
- Mathematical modeling of heat transfer performance and optimum integration into the heat-source/heat-use sequences.

In summary, sustained research and development programs to assure a continuing flow of improvements and innovations, coupled with a growing synfuels industry, would be the most effective insurance the nation can provide against oil-related economic disruption.

Thank you for this opportunity to present my views.

Mr. Fuqua. Thank you very much, Dr. Lee.

Now, we will hear from George Roberts.

STATEMENT OF DR. GEORGE ROBERTS, GENERAL MANAGER, COMMERCIAL DEVELOPMENT, AIR PRODUCTS & CHEMICALS, INC.

Dr. Roberts. Good morning, Mr. Chairman and members of the subcommittee. I am George Roberts of Air Products & Chemicals, Inc.

I appreciate the opportunity to appear before you today and testify at these hearings.

In order to give you a little background and perspective concerning Air Products' testimony, I would like to briefly summarize some of the highlights of Air Products' previous partnerships with the U. S. Government.

During the 1950's and 1960's, when our Nation's missile and space programs required large quantities of liquid oxygen and liquid hydrogen, the Government sponsored research with Air Products and with our competitors, aimed at developing improved technology to help meet those needs.

These fundamental research contracts were then followed by larger scale programs. Subsequently, the building of commercial facilities followed to make these products available.

The technological know-how developed under Government sponsorship was then successfully applied in the private sector. For instance, our merchant hydrogen industry today would probably not be in existence without the stimulus of earlier Government funding.

Second, industrial oxygen which in 1945 cost about $1 per hundred cubic feet sells in the vicinity of 30 cents per cubic feet today, despite the fact that the production of oxygen is a very energy-intensive process. The technology which was developed by private industry with the aid of substantial Government funding resulted in improvements in economies of scale that have benefited all of the consumers of oxygen and the Nation's economy as a whole.

We would like to think that kind of partnership can be extended into the area of synthetic fuels.

I would like to talk about one specific project that we are involved in and which is threatened by present actions. However, before getting into that project, I would like to comment on two questions that are of concern to this committee.

First of all is the proper Federal role in energy research, demonstration, and development. Second, is the effectiveness of past Federal programs in advancing to the specific technologies.

I believe that first and foremost, synthetic fuels are a national issue. The questions of energy availability, sourcing, pricing, are basically irreversibly linked to national security and to the health and stability of the country.

Reducing dependence on foreign crude is an essential element of military strategy, and it has a significant effect on the balance-of-payments picture. It is essential that the Federal Government put policies and programs into place that will lead to long-term addresses and stable programs to develop synthetic fuels based on domestic energy sources, principally coal.

Realistically, the development of a synfuels industry faces an awesome combination of risks: technical, financial, regulatory, environmental, and market risks. In aggregate, those risks are too much for a private-capital structure to bear without help.

Furthermore, even if a synthetic fuels industry were to develop without the stimulus of Federal Government financial assistance, the participation in that industry would be limited to a very small number of today's energy companies.

We believe very strongly that there are smaller, innovative, technologically competent companies that can contribute substantially to synfuels. However, these companies will be effectively excluded from participation in the synfuels industry unless the Federal Government adopts a policy that encourages broad private participation as opposed to a policy that leads to an even higher degree of concentration of the energy industry.

Now, history contains a number of examples of successful partnerships between the Federal Government and private industry. I cited one that came out of Air Products' early history.

The formal text that I have submitted to this committee cites several others.

I would like to talk briefly about an example of an unsuccessful partnership between the Federal Government and industry in which, due to a lack of foresight on the part of the Federal Goverment, a critical technology was not established in time to meet an urgent national need. I refer now to the synthetic rubber industry.

Synthetic rubber was originally developed by the Russians and Germans in the early part of the 20th century. In fact, in the early 1930's, several American companies had picked up the technical leads and they were working on it themselves.

However, there was essentially no Federal Government in place, and the private companies perceived that synthetic rubber would never compete economically with natural rubber. They also perceived significant performance problems. Synthetic rubber did not have the performance properties of natural rubber.

I think some of these lines are very familiar. The essence of it was that private industry, in essence, dropped their research programs in synthetic rubber. In 1940 and 1941, when we found ourselves at war, and when the Japanese swept down through Southeast Asia and cut off our supplies of natural rubber, there was no capacity for making synthetic rubber in the United States.

The Federal Government had not foreseen the need and it had not created the programs that would make that product available to the United States. A crash program was mobilized and it began to bear fruit in 1944 when the Second World War was essentially over.

I would urge this committee not to let the synthetic rubber industry be the model on which the synthetic fuels industry is based.

Turning to a present-day program in which Air Products is involved, I would like to talk briefly about the liquid phase methanol program. It is an example of the need for a consistent Federal policy in synfuels that can allow private industry to make plans to participate in this important national effort.

DOE initiated the liquid phase methanol program late in fiscal year 1981 with Air Products as the prime contractor. For fiscal year 1982, $11.5 million was appropriated for DOE's entire indirect coal liquefaction budget, of which about $4.2 million were to be spent on liquid phase methanol, with approximately the same level of funding in fiscal year 1983.

However, the Administration's recently submitted propsal for the fiscal year 1983 fossil energy budget does not request the funds required to continue the project. Moreover, DOE is presently considering deferral of a substantial portion of the fiscal year 1982 funds. However, I am hopeful that a review of the merits of the project will convince the Congress that terminating this project is not in the best interests of DOE's program and not in the best interests of the Nation.

To summarize the reasons that I feel this, first of all, the project contains a very strong element of fundamental research and generic technology development that is going to be applicable to a host of problems in both direct and indirect liquefaction. It includes research on new families of catalysts, research on the fundamentals of three-phase flow. It also contains a significant component of facilities development, and these facilities will be useful for subsequent DOE programs.

It is structured in a way that makes sense from both a Government and a private sector viewpoint. It uses an existing Government-owned facility in which DOE previously invested $2 million. It has broad private participation from our company; from Fluor Engineers and Contractors; from Chem Systems, Inc. and from the Electric Power Research Institute. This private participation insures that if the technology is successfully developed, it will not languish for lack of interest.

Air Products feels very strongly that the Federal Government has a legitimate role in the development of advanced technologies that will benefit the public as well as the private sector.

Liquid phase methanol is one such technology, and I hope that it will continue to receive your support.

Thank you for your attention and for the opportunity to testify.

[The prepared statement of Dr. Roberts follows:]

24 March 1982
Testimony Before the
Subcommittee on Energy Development and Applications
of the House Science and Technology Committee

Re: The FY 1983 Authorization for DOE's Fossil Energy R&D Program, with specific emphasis on Liquid-Phase Methanol: A Cornerstone Research Project in DOE's Indirect Coal Liquefaction Program

By: Dr. George W. Roberts
General Manager, Commercial Development Division
Process Systems Group
Air Products and Chemicals, Inc.
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Good morning. Mr. Chairman, Members of the Subcommittee, I am George Roberts of Air Products and Chemicals, Inc. (Air Products). My responsibilities at Air Products include both the development and commercialization of new technologies, thus providing a relatively unique overview of, and influence on, the total innovation process. It is a pleasure for me to be with you today, and I appreciate the opportunity to testify at these hearings.

The primary purpose of my testimony is to request the appropriation of an additional $4.13 million in the Department of Energy's FY 1983 fossil energy budget, with these funds specifically earmarked for a very important research project in the area of indirect coal liquefaction, namely the Liquid Phase Methanol (LPMeOH) project.

However, before getting into that specific project, I would like to address two more general issues, which are of special concern to this subcommittee:

- The proper Federal role in energy research, development and demonstration, and;
- The effectiveness of past Federal programs in advancing specific technologies.

In discussing the first of these issues, the focus of my comments will be on synthetic fuels RD&D.

The Federal Role in Synfuels RD&D

Synthetic fuels are, first and foremost, a national issue. Questions of energy availability, sourcing and pricing are directly and irreversibly linked to national security and to the health and stability of the national economy. The American public, as a whole, will be the main beneficiary of a well-planned and well-executed Federal program in synthetic fuels. Conversely, the American public, as a whole, will suffer either military or economic consequences (or both) if the Federal government abdicates its rightful role in synfuels development.

The Federal government has a responsibility to insure that domestically-based alternatives to imported crude oil are developed as quickly as possible. Reducing dependence on foreign crude is an essential element of military preparedness, and could be a positive factor in an otherwise bleak balance-of-payments picture. It is essential that the Federal government create a long-term, aggressive and stable program to develop synthetic fuels based on domestic energy sources, principally coal.

Unfortunately, since World War II research on synthetic fuels in the United States has undergone a number of cycles of emphasis and de-emphasis. These cycles have been brought about by changes in Federal policy and funding, and have been very damaging to the development of synfuels technology. Now, yet another fundamental change in national priority is underway. A new philosophy is emerging concerning the relative responsibilities of the government and the private sector in developing and demonstrating new synthetic fuels technology. However, we must not lose sight of the fact that the responsibility for insuring that the nation has alternatives to imported crude oil rests with the Federal government. Private industry has a legitimate and major role to play in the overall synthetic fuels effort, as a tool for executing technical and commercial programs. However, the overall responsibility for creating and executing a national policy that is in the best, long-term interests of the country cannot be delegated safely to private industry. It is critically important that the government maintain a high level of financial support for RD&D activities in coal conversion and other technologies, in order to insure the availability of the appropriate technologies when they are needed.

While the timing of the requirement for a coal-based synthetic fuels industry is a subject for considerable debate, it cannot be disputed that the country will ultimately need to draw on its vast coal reserves to satisfy its energy requirements. At that time, technology will be necessary to convert coal, efficiently and in an environmentally benign manner, into the liquid and gaseous fuels required by the country's distribution and utilization infrastructure. Government-supported coal conversion R&D programs that are performed at university, industry and government laboratories must provide the data base that is needed to insure that various technologies can be rapidly deployed once the national need materializes.

The U.S. economy will remain heavily dependent on liquid energy sources for the foreseeable future. Therefore, government funding of fossil energy R&D should place a high priority on supporting those advanced technologies that can efficiently provide a substantial supply of liquids from domestic sources. The objective of Federal R&D in this area should be to increase the competitiveness of liquids from coal by establishing an adequate technology base in both direct and indirect liquefaction.

Realistically, the development of a synfuels industry faces an awesome combination of risks - financial, market, technical, environmental and regulatory. In the aggregate, these risks are too much for a private capital system to bear alone. Therefore, the private sector must look to the government for the financial support required to mitigate these uncertainties. Without substantial government financial support for synfuels RD&D, a commercial synfuels industry is very unlikely to develop within the time frame of national need. Moreover, any industry that did emerge would have a very limited number of participants. Without substantial Federal financial involvement, the synfuels industry, if one did develop, would involve only a handful of the largest of today's energy companies. There are many smaller, innovative, technologically competent companies that could contribute substantially to a synfuels industry. However, these companies will be excluded unless enough government support is available to make it financially feasible for these companies to accept the risks and uncertainties that are inherent in synfuels. The nation will benefit if the Federal government adopts a policy that encourages broad private participation in the synfuels industry, as opposed to a policy that leads to an even higher degree of concentration in the energy industry than we have today.

Unfortunately, some critics have voiced a concern that, with its large financial involvement, the government is assuming all of the risks in synfuels development. This view fails to recognize that, in addition to capital, industry is dedicating a portion of its most valuable resource - management and skilled technical people - to the synfuels effort. The result is lost opportunities in other segments, since such human resources are finite and inelastic. Moreover, industry will take on an increasing share of the financial risk once advanced technologies have been developed and proven.

Effectiveness of Past Federal R&D Programs

History contains a number of examples where the Federal government has successfully supported the development of a new technology, which was ultimately utilized to meet a critical national need. Some of these technologies were spun off into the private sector, and produced substantial additional benefits to the nation's economy. For example, the basic aerodynamics research conducted by the NACA (National Advisory Committee for Aeronautics, predecessor to NASA), starting in the early 1920's provided the foundation for design and construction of military aircraft during World War II. In 1943, then Secretary of the Navy Frank Knox offered the following testimonial:

> "The Navy's famous fighters - the Corsair, Wildcat, and Hellcat - are possible only because they were based on fundamentals developed by the NACA. All of them use NACA wingsections, NACA cooling methods, NACA high lift devices. The great sea victories that have broken Japan's expanding grip in the Pacific would not have been possible without the contributions of the NACA."

After World War II, NACA turned its attention to jet engines, and developed the technology base from which many generations of military and commercial aircraft were developed.

Similarly, the longtime commitment of the U.S. Department of Agriculture (USDA) to agricultural research was directly responsible for the development of hybrid corn. This development catapulted the United States into the role of the world's leading agricultural producer, and resulted in more than a tripling of the yield per acre. The first major breakthrough on hybrid corn occurred in 1908, but the full commercial benefits of the development were not realized until the 1940's. Could the private seed companies have developed and commercialized this technology alone, without massive financial involvement from the Federal government? Is it reasonable to argue that the Federal government should not have provided major financial support for the development of hybrid corn because the private seed companies would ultimately derive some financial benefit? Who has received most of the financial benefit from this development, the nation as a whole or a handful of private seed companies?

Conversely, the failure of the government to establish a critical technology has occasionally impaired the country's ability to respond to national emergencies. An excellent example is the absence of a Federally perceived need for synthetic rubber, prior to World War II. Several U.S. companies were involved in research on synthetic rubber in the early 1930's. However, these research programs were gradually terminated in the middle and late 1930's because the anticipated costs of synthetic rubber were too high to compete with natural rubber. In addition, the quality of synthetic rubber products was inferior to those made from natural rubber, and there was little commercial incentive to solve these technical problems. Consequently, when the Japanese swept through Malaysia and Indonesia in 1941, totally cutting off America's sources of natural rubber, the country had essentially no capacity for producing synthetic rubber. During the next several years, a crash program to develop a domestic synthetic rubber industry began to bear fruit, such that, in 1944, when the war was almost over, substantial production rates were being achieved.

The analogy between synthetic rubber and synthetic fuels is frighteningly strong. The U.S. government must take substantial initiatives to prevent history from repeating itself.

Air Products' cooperative efforts with the government date back almost to its founding in 1940. World War II created an urgent need for breathing oxygen for use in high-altitude bombing missions. Air Products responded by designing and building mobile oxygen generators to produce the gas for the government.

During the Cold War, our nation's missile and space programs required large quantities of liquid oxygen and liquid hydrogen for use as propellants. Government-sponsored research and development contracts were awarded to our company to help meet these needs. These contracts were followed by pilot plants and demonstration plants which supplied the needs of a variety of Federal programs. Subsequently, Air Products, as well as its competitors, applied this technological knowhow successfully in the private sector. The tremendous growth of the merchant hydrogen business would not have been possible except as a spin-off of Federally funded R&D.

Industrial oxygen is another specific example of the commercial benefits reaped from government/industry cooperative technological efforts. In 1945, oxygen cost about $1.00 per hundred cubic feet. Even though it is a very energy-intensive product, oxygen now only costs about 30 cents per hundred cubic feet. The technology developed by private industry, with the aid of substantial government funding, resulted in technological improvements and economies of scale which reduced costs and thus benefitted all of society.

I can't promise that kind of cost reduction for synthetic fuels. However, I feel strongly that the technology for converting our nation's vast coal reserves into environmentally acceptable alternatives to imported oil is ready to be developed, demonstrated, and commercialized.

The Liquid Phase Methanol Program

DOE initiated the LPMeOH project late in FY 1981 with Air Products as the prime contractor. For FY 1982, Congress appropriated $11.5 million for DOE's entire indirect coal liquefaction budget. It is our understanding that DOE had planned to spend about $4.2 million of this amount on LPMeOH. Approximately the same level of funding was planned for FY 1983. However, the Administration's recently submitted proposal for the FY 1983 DOE fossil energy budget does not request the funds required to continue the LPMeOH project. Moreover, DOE is presently considering deferral of a substantial portion of the FY 1982 funds appropriated for LPMeOH. Nevertheless, I am hopeful that a review of the merits of the project will convince the Congress that terminating the LPMeOH project will negatively impact DOE's whole program in indirect liquefaction, and is not in the best interests of the nation.

In recent years, the U.S. government has made a strong commitment to research aimed at the conversion of our nation's abundant coal resources into liquid fuels which can be substituted for products derived from imported petroleum. The creation of new technologies that can efficiently convert a wide range of domestic coals into liquid fuels is critical to the nation's national security. DOE's overall program in indirect coal liquefaction, and the Liquid-Phase Methanol project in particular, is highly consistent with this objective.

By way of background, the conversion of coal to liquids, or "coal liquefaction" as it is commonly called, can be accomplished by two different types of processes - "direct" liquefaction and "indirect" liquefaction. Direct liquefaction produces liquids by the reaction of coal with hydrogen at high temperature and pressure. Indirect liquefaction involves the gasification of coal with steam and oxygen to produce synthesis gas, a mixture of hydrogen and carbon monoxide. The synthesis gas is then reacted in the presence of a catalyst to produce liquid fuels. Depending on the catalyst that is used, and the process conditions employed, different liquid fuels can be produced. Indirect coal liquefaction is more flexible than direct liquefaction with respect to the types of coal that can be used and the end products that can be produced, e.g., jet fuel, gasoline, diesel and methanol. However, fundamental technical improvements are essential for indirect liquefaction technology to achieve its full potential.

Historically, most of the emphasis in the coal liquefaction R&D programs supported by the United States Government has been on direct liquefaction. Indirect liquefaction technology has benefitted from government support for coal gasification R&D, since coal gasification is the first major step in indirect liquefaction. However, very little emphasis has been placed on the second major step - the catalytic conversion of synthesis gas into useful liquid fuels.

In recognition of the need for basic, long-range research in the area of indirect liquefaction, DOE recently initiated several programs, including the Liquid-Phase Methanol project. Methanol, the end-product of this technology, holds promise as a clean, premium fuel with a variety of potential applications. It can be used as a transportation fuel by itself, or it can be blended with gasoline. It can also be used as a feedstock to produce MTBE (an octane booster for gasoline) or as an intermediate in the Mobil-M process for producing gasoline. Finally, methanol is a promising fuel for electric power generation, primarily as a fuel for gas turbines. By fulfilling needs in these areas, methanol from domestic coal could become an important substitute for products derived from imported petroleum.

LPMeOH technology has the potential to be a more efficient, lower-cost means of producing methanol from coal than the current commercial processes, which must be licensed from foreign firms and which were designed to produce methanol from natural gas - a premium fuel itself. Based on laboratory work to date, it appears that LPMeOH technology is particularly advantageous when the starting raw material is coal rather than natural gas. Early laboratory results suggest that the liquid-phase methanol process is capable of accepting synthesis gas of varying carbon monoxide (CO) and hydrogen (H_2) contents, and in particular, gases with the high CO/H_2 ratios that are typical of modern, thermally efficient coal gasifiers. It also appears that LPMeOH produces a higher CO conversion per pass, and permits more effective recovery of the heat liberated during the CO/H_2 reaction. However, this potential must be verified and expanded through additional research. More specifically, long-range, high-risk research on catalysts and on novel reactor concepts is required.

The LPMeOH process was originally conceived by Chem Systems Inc. (CSI). Early development work was carried out by CSI under the sponsorship of the Electric Power Research Institute (EPRI). The 42-month LPMeOH program that has been undertaken by DOE and Air Products calls for continued research by CSI, as a subcontractor to Air Products, at CSI's laboratories in Fairfield, New Jersey. Additional research will be carried out at the Air Products laboratories in Allentown and Linwood, Pennsylvania. Larger-scale testing of new catalysts and reactor concepts will be conducted at LaPorte, Texas, adjacent to a synthesis gas plant owned by Air Products. An existing DOE facility, originally built for Liquid Phase Methanation (LPM), will be revamped and relocated from Chicago to LaPorte. Use of this facility, which represents a previous investment of $2.1 million by DOE, is an important feature of the LPMeOH program. The availability and suitability of the LPM facility presents a unique opportunity for the government to conduct engineering research on indirect coal liquefaction in a highly cost-effective manner.

The LPMeOH project contains a very strong element of generic technology development that will be applicable to a host of problems in both direct and indirect coal liquefaction. In the first place, the project involves exploratory research on several new families of catalysts that are very different from "conventional" commercial methanol catalysts. These new catalysts may have performance features that are of interest in other reactions involving synthesis gas. Secondly, the project involves fundamental engineering research on the behavior of three-phase (gas/liquid/solid) reactors. This research will lead to an improved understanding of the fluid mechanics of three-phase systems, which should have ramifications in other coal conversion processes. Lastly, and perhaps most importantly, the project involves the creation of a facility that is large enough to permit the study of the engineering variables that affect process performance. Upon conclusion of the LPMeOH project, this facility will be useful for similar research on other indirect liquefaction technologies. Thus, it constitutes an important element of DOE's long-range research strategy in fossil energy.

The LPMeOH program should not be categorized as just one more project in the DOE fossil energy budget. Rather, it should be viewed as the foundation for a basic, long-range, generic research program in indirect liquefaction. Despite the rather speculative, and certainly long-range, nature of this technology, the fundamental attractiveness of the technical approach has led to strong industrial participation in the LPMeOH program. Air Products, Fluor Engineers and Constructors, Inc., and the Electric Power Research Institute are providing a total of $1.8 million cost share. Chem Systems Inc., the originator of the technology, is not financially able to cost share, but continues as a key partner in the research program.

In summary, there are a number of reasons why the government should continue to support this key project in DOE's indirect coal liquefaction program:

1. Indirect liquefaction is a promising way to convert the nation's abundant coal resources into clean, liquid fuels that can substitute for liquid fuels produced from imported petroleum. Such a capability must be considered an essential element of national security.
2. The LPMeOH project is basic to DOE's overall program in indirect liquefaction. It contains a large component of generic research and facilities development that will be broadly applicable to several "downstream" indirect liquefaction programs.
3. Methanol, the end product of the LPMeOH technology, holds promise as a clean, premium fuel in a variety of applications. Methanol from domestic coal could become an important substitute for products derived from imported petroleum.
4. The LPMeOH technology has the potential to be a more efficient, lower-cost route to methanol than conventional gas-phase processes.
5. The LPMeOH project is structured in a way that makes sense from both a government and private sector viewpoint. The program uses an existing, government-owned facility, in which DOE previously invested $2.1 million. The broad private participation indicates that this research, if successful, will not languish for lack of interest or capability.

Air Products feels strongly that the Federal Government has a legitimate role in the development of advanced technologies that will benefit the public as well as the private sector. LPMeOH is one such technology which we hope will receive continued support.

Thank you for your attention and the opportunity to testify before your subcommittee. I will be glad to answer any questions you may have.

Mr. **Fuqua**. Thank you very much.

Mr. Dunn, do you have any questions?

Mr. **Dunn**. Thank you, Mr. Chairman.

I have one question of a general nature. Each and everyone of the witnesses in one way or another has talked in rather disparaging terms about the regulated industry.

My question is, because of those questions, do you favor deregulation? As an example of what I am talking about, Mr. Webb—and I quote—says, "A little over half of our primary energy finds its way to consumer through electric and gas utilities, and the utilities are regulated, price-controlled industries selling their product not at a free market price but at price-controlled prices. The consequence of that is consumers of gas and electric have less of an incentive to conserve and therefore to undertake research on conservation."

Do each and everyone of you gentlemen support deregulation of the natural gas industry?

Mr. **Lawrence**. I think that we probably have the longest track record in that regard, Mr. Congressman. We have long, for some 12 years, supported the deregulation of the fuel price of new gas to stimulate exploration and drilling and improve the supply situation.

The Natural Gas Policy Act of 1978 was a compromise and a phased decontrol which does move toward accomplishing that and it has worked very well.

Mr. **Dunn**. Do you favor speeding that up?

Mr. **Lawrence**. As to new gas, yes. As to old gas, we have some serious concerns, because it can have a serious comsumer price impact without the concurrent supply benefits. Therefore, we are very reluctant to support any accelerated decontrol of old gas.

I should draw one distinction here now. I think in the way that much of our research testimony is discussed, we are not in any way talking about deregulating the utility role of gas distribution companies or the regulated role under the Natural Gas Act of natural gas transmission companies. We are talking about only deregulating the fuel price of the producer's product that comes into our system.

Mr. **Dunn**. Do you concur?

Mr. **Webb**. Yes, Mr. Dunn. Let me add one thing, though, I think needs to be pointed out here. As the research arm for the regulated segmemt of the gas industry which would continue to be regulated under any of the wellhead deregulation phases, that puts additional pressure on us to put as much of our research as we can into the near-term areas to try to get a technology base in place to offset the decrease in price advantage of gas. It makes it even more important that the Federal Government continue to support the long-term, high-risk reserach which we cannot simultaneously pick up in the near-term.

Mr. **Dunn**. I have a question, and Dr. Lee and I discussed it at length before. Could Mr. Lawrence comment on peat? What is your comment on that? Is that a viable source? I know that my State and several other States in the Northeast and Midwest have an abundance of it. I have been impressed with the research going on there in Chicago.

You didn't touch on that in your testimony. Could you give me an opinion on that?

Mr. LAWRENCE. Well, again, we are very hopeful, and we do have some portion on peat in our $104 million. I think that the answer is yes, it has a tremendous benefit in having such a wide geographical location in States that are not conventional energy-producing States of either coal or hydroelectric for that matter or certainly oil or gas.

We have some member companies which have devoted a good segment of their own funds to peat research, and they are very encouraged about it.

Therefore, we would certainly share Dr. Lee's enthusiasm for its prospects.

Mr. DUNN. I have one final question. Mr. Webb, you talked about an increase of R. & D. this year of $29 million from the utilities. I saw an article in the New York Times on Monday talking about the utilities in general having been a good buy for the last year and looking to be even more of a good buy in terms of a stock.

Those two statements seem to fly in the face of what my own utility company tells me about just how tough it is. I can't make those two statements jive—being a good price and finding an extra $29 million for research, and then on the other hand talking about the difficulty of funding.

I guess my question is where is this $29 million in funding coming from? Are utilities a good buy or not? I should tell my broker that, too. [Laughter.]

Mr. WEBB. I think whether utilities are a good buy or not depends on the regulatory climate in their particular region, sir.

Let me try to explain what I am talking about. The $29 million increase for near-term research we are talking about proposing to our board for 1983 is based on a plan we will submit to the Federal Energy Regulatory Commission. The Federal Energy Regulatory Commission and the 50 States are automatic intevenors on the process. If they approve it, then the funding unit to pay for that research is applied to the interstate pipeline systems and the volumes of gas sold. It is, therefore, a pre-approval process to fund research across the entire gas——

Mr. DUNN. No, the nature of my question was where does that $29 million come from. Was that the result of the tax package last year that you are going to increase funding this year at that level in R. & D.?

Mr. WEBB. No, sir. It is laid out in our 5-year plan we submit to the FERC. It is the second year of a plan approved last year.

Mr. DUNN. I thank you, Mr. Chairman.

Mr. FUQUA. Mr. Volkmer?

Mr. VOLKMER. I thank you, Mr. Chairman.

I think I know what the answer is, but, Mr. Webb, can you tell us what would happen if we are not able to increase the funding level above the administration's budget on the research programs that we have?

I understand that the Gas Research Institute has some cooperative programs. What would happen to those R. & D.'s?

Mr. WEBB. Mr. Volkmer, what would happen to the long-term programs is some of the work we are doing with them in geopressured methane, the multiwell experiment for the lenticular sands in the west, the eastern mineback program in the Devonian shale, and the other long-term research programs would be terminated because of the $33 million we are adding for 1983, $20 million is to offset a lack of DOE funds or a termination of DOE funding in the near-term programs.

Mr. VOLKMER. All right.

Mr. WEBB. So the long-term research programs would be terminated.

Mr. VOLKMER. And so basically they would be lost until we could find the funds somewhere.

Mr. WEBB. Yes, sir, until we get through this transition period.

Mr. VOLKMER. Do you believe that the Gas Research Institute would be able to get back into it at a later date—get back into those programs with sufficient funds on your own?

Mr. WEBB. That I don't know. We would certainly look at it, but we would not be talking about getting back into it for the next 4 or 5 years because we have got this tremendous transition. As the differential between the gas prices and other energy prices narrow, there is tremendous market pressure on the gas industry to try to get more efficient end-use appliances and near-term research. Therefore, for the next 4 or 5 years, the answer is no, sir. In the long term, I am certain we would have to look at it.

Mr. VOLKMER. On the near term are you talking about a more efficient use of gas?

Mr. WEBB. Yes, sir. We are talking about gas heat pumps, more efficient gas furnaces, and industrial cogeneration. We are talking about on-site fuel cell systems, those sorts of devices.

Mr. VOLKMER. Those sorts of things, but not basically production alternatives?

Mr. WEBB. In the unconventional gas area, we are talking about doing research in the field-test phase which is the more near-term phase, but we are not talking about research where we are going into a formation, to conduct research to get generic data that is applied for all of the industry. That is the sort of long-term research we will not be able to do because we cannot get industry cofunding.

Mr. VOLKMER. I understand.

Mr. Lawrence, I would like to ask a question. What do you foresee the American Gas Association, as a result of gradual gas deregulation—much increase in supplies will we have in natural gas?

Mr. LAWRENCE. Our gas supply committee completed a comprehensive 2-year look at some 17 different sources of methane energy. Into the year 2000, depending on public policy directives, we see an increase in gas supply from the range of 10 to 50 percent in the year 2000.

Now, even with the accelerated deregulation of new gas field prices, which are indeed creating new exploration drilling stimulus, we still see a long-term eventual downward trend in conventional lower 48 State supplies from the present level of 19 and a half trillion to the range of 14 trillion in the year 2000.

Therefore, that growth comes from coal gasification, Alaska gas, Canadian, some of the new technology sources particularly in the tight sands area.

However, in different scenarios, we project that growth range of from 10 to 50 percent.

Mr. **Volkmer**. How much is coal gasification a part of that?

Mr. **Lawrence**. These are high-low scenarios. It would be between 3 and as much as 10 percent of the total in the year 2000.

Mr. **Volkmer**. Yet we have only got one plant underway right now.

Mr. **Lawrence**. One commercial project is underway in North Dakota. The high side projection over the next 18 years would contemplate 33 such plants, but that is indeed a high side projection.

Mr. **Volkmer**. Yes, it is.

Thank you very much, Mr. Chairman.

Mr. **Lawrence**. Could I just add a brief comment, Mr. Volkmer?

Mr. **Volkmer**. Yes.

Mr. **Lawrence**. On the projects that Mr. Webb ticked off that would be terminated——

Mr. **Volkmer**. Yes.

Mr. **Lawrence**. If this administration budget is implemented, each one of those dramatically fit the administration's own criteria for the type of Federal research that we should be involved in.

Mr. **Volkmer**. I realize that. We are finding that out all through out energy budget though. We are finding that a lot of these long-term, high-risk projects are being terminated.

What concerns, I think, many members of the committee—at least it does me and I have heard others express it, too—is that these will not be continued in the private industry in general.

Therefore, we are abandoning basically some of the things that we started up since especially in 1973 and 1979. Some of us worry about where we are going to be in that year 2000.

Mr. **Lawrence**. Right. I think your concern is well placed.

Mr. **Volkmer**. Thank you. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, Mr. Volkmer.

Mr. Roberts, you mentioned in your comments about the threat of present action. Does that mean a $1 million deferral, a rescission?

Dr. **Roberts**. When the fiscal year 1983 authorization came forward, DOE recognized that the project was not included in the fiscal year 1983 budget. It in essence has proposed a deferral of all of the funds other than those required to determine the project——

Mr. **Fuqua**. Yes.

Dr. **Roberts**. For fiscal year 1982.

Mr. **Fuqua**. And what is this going to do to your project?

Dr. **Roberts**. Well, it is effectively going to terminate it. There is no way that the project could be continued without DOE support.

Mr. **Fuqua**. Dr. Lee, is there any deferral spending on the peat project?

Dr. **Lee**. There is a $100,000 item put up in the deferral package. The large scale pilot plant program on peat which I testified on last year has been completed. As a matter of fact, I have disbanded my staff because there are no funds coming in 1983. Some of this technology gap that I am talking about has already occurred.

What we are now talking about is a reasonable amount of funds to close out and adequately deactivate this large pilot plant.

What we are recommending is continued effort on a long-range resource assessment and developing the necessary harvesting techniques for peat just to have that technology ready when we might need it some day.

We have been told that the large-scale, around-the-clock, significant size programs will not be in the cards. Therefore, there is $100,000, as we understand, which is being put up to take 1982 money out.

Mr. **Fuqua**. There are those who say that history repeats itself. Mr. Volkmer has pointed this out on numerous occasions when we have had witnesses testifying in particular about fossil about the attempt in the 1950's to begin a synthetic fuels industry in this country, and it failed.

Do you see any parallels now with the way we are moving in funding, particularly in some of our R. & D. programs?

Dr. **Lee**. Unfortunately, I do. Coming from a background which relishes in history, we see human history to be replete with repeating mistakes of the past. I think I particularly enjoyed Dr. Roberts' comment about the synthetic rubber industry.

Mr. **Fuqua**. That was a very good parallel there.

Dr. **Lee**. Not only that, but we seem to take that as an example of how we solved problems, but it should be taken as an example of how we got ourselves into a mess without anticipating the problem.

I certainly would like to see us not be involved in that again. All these efforts—whether it is enhanced gas, enhanced oil, methanol, or synthetic fuels, it is impossible for the private sector to make long-term decisions on anything that is long term without having a policy set up by the Nation.

The amount of money is large, to be sure, but in addition to the amount of money that is involved, it is the uncertainty of the world, the ups and downs of the oil pricing which is absolutely unrelated to the market atmosphere and is related to National feelings and policies.

I am sure that what we are moving toward is a deemphasis of energy supply R. & D., expecting the so-called free market to take care of our problems, and it will not. I think that you and other Members of the Congress must speak out and take action to make sure that there is a well-balanced effort to assure ourselves of adequate long-term energy.

Mr. **Fuqua**. Mr. Lawrence, you mentioned in your statement—and I may not be quoting or paraphrasing it quite accurately, and if I am not, please correct me—you said something that there were those areas of R. & D. that had high priorities that the Government should fund, and there were those that they shouldn't be involved in.

How do we set the priorities so that we do fund those?

We have had witnesses. We had one yesterday who was very sincere. He said that he felt like that the Government was probably an impediment to private enterprise in developing these.

Now, I know that the gas industry is a regulated industry. The utility industry is regulated. You mentioned that in your statement.

It seems to me that in those where we have had—and we had hearings last year where many of the utilities came before the committee and we were examining the impact of new technology being put on line. Many of them said that they were unable to do it because of the rate base that they have, when they sell their bonds for new development, they have to go with something that has been proven for the last 50 years.

How do we, in carrying out our responsibilities try to address that? What is appropriate? We certainly do not want to in any way diminish the interest of private industry to develop. That is what we are trying to encourage, I hope. However, we have to do some of the basic research and the demonstration that is of high risk in order to get to that point.

How do we do this? Can you help us provide some insight in how we may be able to do a better job?

Mr. LAWRENCE. I think, Mr. Chairman, yours is the ultimate question, in this day of fiscal austerity and very tight Government spending, that I think has occupied everyone's attention. I think the administration's criteria that they have laid down are very good. Research to be funded should require a long-term research effort that is high risk, and above all, that has a high potential if you are successful.

We have tried to pare our list to that end. Our biggest frustration is in the whole energy research budget proposed not just by this adminstration but by so many preceding administrations is this almost total preoccupation with the nuclear panacea. I think that it is a mistake to be relying on any one particular energy solution——

Mr. FUQUA. Yes.

Mr. LAWRENCE [continuing]. However exotic or romantic it might be.

Mr. FUQUA. I couldn't agree with you any more.

Mr. Webb, the proposed budget that I think all of the witnesses are supporting which is the $104 million budget—I don't have to remind you that the administration, for those items collectively, has only recommended $19.8 million.

Do you consider yourselves big spenders and wild with money?

Mr. WEBB. No, Mr. Chairman, we do not. The problem we face, as I stated earlier, is we had met with the administration last year when they were formulating their policies, and we tried to work out with them orderly transfers of the near-term programs.

For instance, picking up the field test of the onsite fuel cells, picking up all of the work in gas-fired heat pumps and furnaces. I can go down the list. We thought we had made a successful transition. We literally thought we were in a good position.

We went to our board of directors. The board of directors said, "We take the lead in the near term. Continue to follow the DOE lead in the long-term, high-risk research."

Then when the budget was submitted, it seemed the administration had redefined the definition of what is long-term, high-risk research, and I think their definition is not in reality with the way industry looks at making research investment decisions.

Therefore, the problem we have now is the transition cannot occur overnight.

Mr. FUQUA. Yes.

Mr. **Webb**. In a regulated industry, I think you even have a larger response time than in an unregulated industry in that if they change the policy every 6 or 9 months, there is no way we can do adequate planning or contingency funding.

Mr. **Fuqua**. There are those who propose that we freeze spending at fiscal year 1982 or 1981 levels. Someone recommended to me that we go back to fiscal year 1980 spent funds. I would like to point out in this area that in fiscal year 1981 there was almost $300 million spent in these various areas that are being requested at almost $20 million this year—$19.8 million.

Last year or in the fiscal year we are in right now, we are spending $119 million that has been appropriated. Therefore, this is still below the fiscal year 1982 level which is what you are recommending.

Mr. **Webb**. Yes, sir. Not only, as the others pointed out, is it high risk, long term, but we think it is consistent with the constraints on our economy. It is an actual reduction over previous years' appropriations. In fact, I think in fossil energy as a whole, you have to go back to 1972 to get back to the research budget levels being proposed by this administration.

Mr. **Fuqua**. That is when we thought that we had plenty of crude oil to last us—it was perceived that way, I think, by many people—on and on, and that there was not any problem until the fall of 1973 when we found out differently.

Mr. **Webb**. Yes, sir. I would like to add one thing, Mr. Chairman.

Mr. **Fuqua**. The prices were a lot different, too, back then.

Mr. **Webb**. Yes, sir. They were. However, the concern I have is that with the declining oil prices now—and as you mentioned, gasoline may be at $1 a gallon—it is getting lulled back into the false security in that we have taken a look at——

Mr. **Fuqua**. I don't know where that $1-a-gallon gasoline is. I haven't found any yet, but I keep reading about it.

Mr. **Webb**. Yes, sir. All I was going to say was that we recently have done a study in looking at the electric utility industrial market that uses gas and how much of it has dual-fuel capacity out there right now. In other words, they have previously burned either oil or gas, and it pretty well fluctuates on market prices. There is presently gas used in facilities that have dual-use capacity—dual-fired capacity—of 6.3 trillion cubic feet. This is in 1981. Now, that is a little over 3 million barrels of oil per day.

The danger, if we don't develop the technology base and continue the long-term, high-risk research, is if that market swings back to oil then in fact you are pulling 3 million barrels a day out of the world oil market, and that tends to drive back the price of oil and increase the pressures on inflation.

Therefore, we are in a vicious cycle here. If we are not careful we will make the same mistake. As you said, history repeats itself.

Mr. **Fuqua**. I want to thank all of you for being here this morning.

Mr. **Dunn**. Mr. Chairman, may I ask one more question?

Mr. **Fuqua**. Surely, Mr. Dunn.

Mr. **Dunn**. As one who supports nuclear energy on this committee, I still share the concern of the majority in the committee of the concentration on nuclear research in this year's budget.

If you were the administration—you are experts on energy—how would you justify the inconsistency that we support research in nuclear but not in other areas? How would you justify that if you were the administration?

Dr. **Lee**. The researchers are bold and stupid. They will speak up first. [Laughter.]

I think the justification can only be made, one, on the glamor and the exotic nature of nuclear. I speak as a person who got his Ph. D. degree working in nuclear fuels so I have nothing against nuclear.

The misconception in my mind is that the nonnuclear aspects—whether it is coal, gas, or oil—each have an operating, producing private industry to take care of us. The tendency seems to be: Well, let those people take care of it.

The nuclear, because the Government is still involved in getting the uranium to the point where the utilities can make use of it, that resource—that development of resource, the production part, sits in the Government's hands. I believe it has a lot to do with justifying the whole thing.

You could get just as exotic with gas supplies in the supply book which Bud talked about which looks at all the long-term options which are just as far-reaching as the nuclear option. However, it has been relegated to the industry to take care of, and Government has opted to avoid that issue.

Mr. **Dunn**. The only justification that I could get was that it was in Senator Baker's home State. I didn't like that one. I thank you, gentlemen. [Laughter.]

Mr. **Fuqua**. There is some going on in Michigan, I think, too. [Laughter.]

Thank you very much. We appreciate you being here this morning.

Dr. **Lee**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Our next panel of witnesses will be: Mr. William Podolny, general manager of fuel cells operations of United Technologies; Dr. James Gumnick, executive vice president of the Gulf Universities Research Consortium; Mr. Robert Roach, the director for the synthetic fuels project at the Environmental Policy Center; Mr. Tobias Anthony, the director of business development and Washington affairs, Research Cottrell; and Dr. Joseph Yancik, vice president, research, National Coal Association.

We will begin with Mr. Podolny.

As we did with the previous panel, maybe some of you were not in the room, if you want to submit your statements—or the statements that have been submitted—will be made a part of the record and if you want to summarize that would certainly be welcome.

STATEMENT OF WILLIAM N. PODOLNY, VICE PRESIDENT AND GENERAL MANAGER, FUEL CELLS OPERATIONS, POWER SYSTEMS DIVISION, UNITED TECHNOLOGIES.

Mr. **Podolny**. We appreciate the opportunity to comment on the budget outlay for fuel cells recommended by the administration.

We want to compliment this committee because of its previous actions in the field of fuel cell powerplant development.

Two major programs are now underway for the application of new generators that will have a constructive benefit to our Nation. I am referring to the 4½-megawatt electric utility powerplant which is being constructed in New York at a Consolidated Edison site, and the recent decision to go ahead with large-scale demonstration of 40-kilowatt fuel cells by the natural gas industry, as Mr. Webb referred to earlier in conjunction with the Gas Research Institute.

Both those programs will provide the basis for establishing the constructive benefits that we can look forward to from fuel cells. This committee has to be complimented for its support of those programs.

We are concerned about the future. There has been a lot of testimony that documents the administration's position, vis-a-vis the free market and the responsibility of the private sector. As a private sector corporation, we do not have any major differences with the administration. I will remind this committee that the private sector, United Technologies and electric and gas utilities, carried the major effort to evolve the fuel cell powerplant until 1975, and we developed and tested the first laboratory demonstrations of a large-scale, 1-megawatt powerplant as well as a small-scale 40-kilowatt powerplant with private sector resources only. All that activity preceded Federal funding with the advent of the requirement to conform to the changing policies and conditions associated with Federal development of energy resources and Federal regulation of energy use.

We continued our fuel cell program effort with the sense of responsibility that we demonstrated during the private sector activity. I would like to take a moment to outline our strategy to commercialize the large-scale fuel cell powerplant for electric utility requirements, the small-scale powerplant for gas industry requirements, and the use of the fuel cell by the chemical process industry designed to make a constructive contribution to conservation, better use of our resources, better use of our capital, respect for the environment and better economic performance through lower prices for end-use energy.

Our approach is to work with the electric utilities to structure a three-tiered program. We have started negotiations with a small number of pioneering companies who will elect to purchase prototypes of the commercial powerplant with a reduced warranty for delivery in the mid-1986–87 period at premium prices as high as two to three times what is considered an acceptable, competitive price. We are appealing to pioneering companies to accomplish this first act of commercial installation for the fuel cell powerplant. This would be followed by an order for additional powerplants for delivery in the 1988 to 1989 period, either in the hands of many utilities or utilities who would buy multiples. Once again, the customers would buy at a premium price and operate equipment with limited warranties.

The parties to the first and second powerplant deliveries share a mutual concern to provide for the readiness of this equipment and to insure the readiness of the utilities to use this equipment and to provide for the readiness of a manufacturing structure to supply that equipment. We see a major window of opportunity some time in the 1988-90 period where we think there will be a severe shortfall of electrical generating capacity and a real disruption in the ability of the Nation to respond with cost-effective, efficient, sitable, and environmentally satisfactory equipment.

The program does, in fact, carry a large-scale, private sector, manufacturing risk in the first portion of the activity through 1986. We are working with other manufacturers and trying to establish a joint venture to help share that risk, although our negotiations with the utilities are based on the assumption that we are going ahead solely on their commitment. Our timetable is to go forward with the project beginning in 1983, and we are currently negotiating with several utilities.

The equivalent program for the gas utilities is a commitment we have made to the Gas Research Institute, the gas utility users organization, and individual gas utilities. As we start to deliver the first of the 40-kilowatt powerplants, and as we see the results of the technology programs that Mr. Webb has referred to, we will jointly define what we consider to be the proper commercial configuration for early entry to the gas industry. Our strategy and commitment to that industry is to undertake an equivalent program with a bona fide offer by the private-sector manufacturer to a group of gas utility companies who would commit to a threshold minimum order. Early equipment would be sold at a premium price, and gas utilities would accept less-than-normal warranties.

All of this begs the question of the proper position of the Federal Government with respect to continued support for fuel cell technology. These two commercialization activities, both large scale and small scale, and a third which is now underway with chemical process companies, are true high-risk ventures not only in a commercial sense but in a technology sense as well. At best we will be entering the marketplace with what I would call early entry equipment just barely passing muster with respect to satisfactory economics and satisfactory durability and reliability.

We look to the continued and long-range Federal support of base technology activities which would really improve and verify the full range of benefits of fuel cell powerplants by providing for a more stable and fundamental understanding of higher efficiency, lower cost, greater durability, higher reliability, and greater fuel flexibility. If it is not forthcoming, I think we will stay out there in about 1990 with a massive capital commitment on the table and a seriously limited possibility of penetrating the marketplace.

The present administration's fuel cell budget is a substantial reduction from the 1982 budget. This committee, in its wisdom in the past, argued that there should be competition in the research on fuel cell technology: competition both in the near term and the long term. I do not think we have ever disagreed with the principle of developing a competitive capability in the United States; but given the present administration budget, it is hard to see how the Federal Government can support continued activity of the two critical demonstration activities—the small machine program with the gas industry, the 4½-megawatt machine in New York—and carry a large, multiorganization, competitive structure in fuel cell technology. I think there are just not sufficient funds available for this technology activity if we still hope for strong, active pursuit of the commercialization of powerplants by the leading manufacturing structure. We consider that to be a serious effect of a reduced budget.

The second effect of a reduced budget that we are concerned with is the fact that both the 4½-megawatt installation in New York and the small machine activities are, at best, experimental programs with vulnerabilities. Therefore, you can expect to see contingency requirements. We fully expect to have some difficulties: I think everybody should recognize that. We must recover from them, and the resources to provide for that recovery should in some way be provided for in the budget so that the past investment is honored.

Most important, if we are going to undertake this large-scale commercialization effort—and the private sector is going to take this step—I think that the bulwark of consistent pursuit of base technology by the Government is almost a necessary prerequisite to get the participation by the utility companies.

We thank you very much. We would like to see this committee at least support the 1982 level of appropriated funds for fuel cells. Of course, we think that is just barely adequate.

[The prepared statement of Mr. Podolny follows:]

Testimony of

William H. Podolny
Vice President and General Manager
Fuel Cell Operations, Power Systems Division
United Technologies Corporation

before the

Committee on Science and Technology
U. S. House of Representatives

March 24, 1982

Mr. Chairman and members of the Committee, thank you for the opportunity to appear before this committee to address the adequacy of the Department of Energy's fuel cell budget. Let me begin by acknowledging the key role this committee has played in providing the funds required to execute the DoE fuel cell research and development program now in place. This includes significant cost-shared support of field testing of an experimental 4.8 MW electric utility system in New York, and similar support for the evolution and field testing of on-site fuel cell energy service in cooperation with the natural gas industry. The federal government, in recent years, has become a valued partner in continuing private sector efforts to develop fuel cell technology. Today, I would like to address the issue of the appropriate federal role in future fuel cell research and development from the perspective of a participant in the private sector effort that began at United Technologies in 1959.

United Technologies has accepted the challenge to bring fuel cell powerplants to the marketplace with the resources of the private sector. I will outline our plan to accomplish this task in the course of my testimony, but let me say at the outset that the success of this venture may very well hang on the willingness of the federal government to assume a necessary and constructive role in supporting high risk, long payout fuel cell technology base activities. This fundamental research and development buttresses the ability of the private sector to take these research results and apply them to new products and processes. Without this mutually beneficial division of responsibility, the private sector will be unable to perform its proper role in commercialization.

Let me briefly describe the fuel cell powerplant. In it a wide range of existing or planned fuels are converted to electricity and useful heat more efficiently and with less environmental impact than any other form of energy conversion. The powerplant chemically transforms the raw fuel to hydrogen; converts hydrogen and oxygen from the air to dc power; and, converts dc power to grid-quality ac power. Heat generated by the conversion process can be recovered for cogeneration applications. Suitable raw fuels include light petroleum distillates, natural gas, coal-derived gases and liquids and gases produced from biomass and some industrial by-product gases.

Because the fuel cell's electrochemical conversion is static and essentially independent of scale, the system is uniquely efficient over a wide range of power ratings and at part-power operation. Noise and emissions are dramatically lower than combustion-based generators and external water is not required for fuel processing or cooling. With these fuel, siting and operational features, fuel cells provide electric utilities with an important option for today's uncertain business environment.

The fuel cell powerplant can also address a critical economic problem if it is introduced into commercial service in a timely manner. The cost of energy service has been rising at a rate in excess of the general inflation rate in recent years. Because utilities do not have the same unregulated price and tax benefits provided to the rest of the private energy sector, they have little discretionary capital to invest in efficient equipment. As a result, the costs of all goods and services with an energy content are needlessly inflated. Incremental additions of fuel cell equipment in the 1980s and 1990s can begin to address that inefficiency.

I. Private Sector Commercialization

As this committee is aware, United Technologies has been involved in a number of private and government-funded experimental and developmental fuel cell powerplant programs during the past decade. I will limit my observations to our attempts to provide a commercially acceptable, phosphoric acid, electric utility generator because this program provides a clear illustration of the appropriate role of federal government in supporting energy research and development.

During the past eighteen months United Technologies has been developing an all-private-sector program to commercialize electric fuel cell generating equipment for electric utilities. Changes in federal policy, together with significant accomplishments in the technology and powerplant system design, have resulted in an explicit strategy to make commercial fuel cell powerplants available to the industry before the end of this decade. The critical elements include:

a. Essentially an all-private-sector endeavor.

b. A product development and verification program culminating in the delivery of early commercial units to at least 3 utilities in the 1986 period.

c. A concurrent commitment by utility companies for a minimum number (approximately 20) of prototype production units, with delivery starting in the 1988 period.

d. The participation of a major electric utility equipment designer and manufacturer as a venture partner to enhance the success of initial equipment deliveries and to provide long-term support and service for future commercial sales.

e. Continued strong technology research and development support by the U. S. Department ~~of~~ Energy and the Electric Power Research Institute at least through the point at which successful operation of the three early -delivery units is achieved to complement what would otherwise be a private sector activity.

Progress is being made toward identifying the initial utility applications for a nominal 10 MW powerplant and discussions are going on with several potential venture partners. Our goal is to have both of these relationships established by the end of this year.

There are three points of the plan that should be emphasized. First, it represents a strategic objective that will evolve over time in response to changing conditions. Second, the business and technical risks to be borne by the early-delivery utility customers and to the venture partner are extraordinary. Third, the technology base support of the federal government is an integral and essential part of the program.

Participating utilities will be asked to pay a premium for early delivery units under the program. They can justify this investment, in part, on the basis of the fuel cell powerplant's projected performance in the grid system. In addition to providing high-efficiency, fuel and siting flexibility, and low emissions, the fuel cell powerplant can reduce transmission and distribution costs, reduce reserve margin requirements, improve real power response, improve reactive power control, and provide one of the best options for industrial cogeneration. In addition to these operational benefits, the powerplant will be modular and factory-assembled, designed to minimize on-site construction costs and reduce lead times with attendant financial benefits to utilities. At present, raising new utility capital is difficult because of the large percentage of assets tied up in construction work in progress and the resultant large percentage of income derived from allowance for funds used during construction. Fuel cell powerplants permit incremental, short lead-time additions to capacity, thus easing the financial squeeze.

Despite this impressive list of potential benefits, the technical and business risk to the utility participants and venture partner remains substantial. These high-level business risks are appropriate for the private sector to assume, but they make sense for the participants only if the technology base for phosphoric acid fuel cells is assured of continuing federal support. While commercialization, with its attendant business risks, remains an exclusively private sector activity, technology research, with its focus on reducing technical uncertainty, is an appropriate function of the public sector.

II. Technology Base Support

The utility marketplace requires reliability, durability and competitive cost in new equipment. Although phosphoric acid fuel cell technology has seen substantial strides in recent years, a great deal of technology work remains to be done. The materials on which fuel cell technology is based are limiting factors on performance, durability and cost. Present configurations push existing materials to the limits of their capability. For example, the fuel reformer is constrained by the temperature limits of reformer tube materials; the cells are limited by the catalyst layer and catalyst structure; and the inverter and controls are limited by the applicability of new solid-state materials and devices.

The fundamental task of a base technology program is to increase the range of available options by testing new materials, adapting existing materials to new uses, and applying new and existing materials in new configurations. Each of these candidate materials must be exposed to the full range of experimental conditions, including different corrosive environments, widely varying pressures and temperatures, and normal cycling loads. In addition, materials must be shown to have the endurance and performance required for years of continuous utility service.

No private sector commercialization program can thoroughly address these basic research tasks. Nevertheless, they are essential in the evolution of fuel cell technology. The range of technical options is a direct function of federal generic research funding levels. The breadth of those technical options is the primary factor in establishing an acceptable technical risk for private sector participants in commericalization. The larger the federal investment, the higher the confidence of the private sector that the technology improvements will, in the future, become available for commercialization.

III. Private Sector Market Pull: The Window of Opportunity

The potential market for fuel cell powerplants in the late 1980s is indicated by the data of several recent utility market projections. Perhaps the most interesting recent study is a DoE examination of financial constraints on the electric utility industry. The study found that under a financially-constrained case in which utilities could finance only 50 percent of coal and 80 percent of nuclear plants planned or under construction, as much as 50 gigawatts of other generating capacity would be required by 1995 to meet peak demand. That demand could be met most efficiently, and ultimately at lower cost to the consumer, by fuel cell powerplants.

If the electric utility fuel cell powerplant were to achieve full commercial availability by the late 1980s, it would be in a position to compete for this market. Clearly, the time has come to position fuel cells for the market place. While the federal government has no formal role in responding to the market, an appropriate level of federal support for base technology activities might allow the fuel cell to be "pulled" by the market into commercial service.

IV. Tax Credits

The committee staff has asked if the recently enacted R&D tax credits in the Economic Recovery Tax Act of 1981 will alleviate the need for federal research and development support for fuel cell technology. Because the tax act limits those credits to new investments above an historical R&D baseline investment, it is difficult for United Technologies to take advantage of the provision. Last year our corporate investment in discretionary R&D, exclusive of direct federal support, was in excess of $730 million. This investment ranks among the top for U. S. industry. The R&D credit does not, therefore, generate sufficient new funds to make up for reductions in federal R&D budgets.

Similarly, tax benefits to the utility sector are of limited value. The ability to depreciate utility property at almost twice the rate of its effective life under the recent Tax Act should improve utility cash flow. Utilities with major construction requirements, however, generally pay low taxes already because of the normal 10 percent investment tax credit. The current book tax rate on electric utilities is approximately 40 percent, while actual effective tax payments are about 7.5 percent. By the second year of the phase-in of more rapid depreciation, the revenue impact on the electric utility sector will exceed the present level of tax payments.

V. Conclusion

United Technologies is engaged in a good faith effort to bring a new technology to the market place. We have accepted the challenge to create an all-private-sector program toward that end. Our program calls on all parties in the venture to assume extraordinary financial and technical risks in bringing the fuel cell powerplant into commercial service. These parties cannot be expected to exceed the limits of prudent business practices. For potential customers to responsibly accept the risks involved in early delivery powerplants, it is crucial that federally-supported technology base activities continue on the underlying fuel cell technology.

We are convinced that a fuel cell powerplant can be extremely attractive to utilities in the years to come. We believe the advantages of short lead time, cost, siting flexibility, energy efficiency, and other factors will prove to be extremely valuable to the utilities and their customers as they seek to meet projected load growth in a difficult financial environment. These advantages will not, however, be considered in a vacuum. Unless the utility has had experience with the technology or has access to other utilities that have used the system, it will lack the confidence to invest in a fuel cell powerplant.

We firmly support the Administration's general economic recovery program. Within that framework, the federal government can play a legitimate and valuable role in support of private sector technology development by sustaining fuel cell base research. White House budget documents argue that the "Government can perform an important role in support of private sector technology development by focussing federal funding on technology base research with widespread application (e.g., materials research or understanding the mechanisms of catalysis)..."* What we seek is implementation of this White House policy through adequate funding of fuel cell technology. Continued funding at the FY'82 level of $34.5 million for fuel cells, with $21 million for phosphoric acid technology base activities, may make the difference between success or failure of the private sector's program to bring fuel cells into commercial service.

Thank you for this opportunity to appear before your committee.

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*Executive Office of the President, Major Themes and Additional Budget Details, Fiscal Year 1983, p.98.

Mr. FUQUA. Thank you very much.

Dr. Gumnick, your prepared testimony will be made a part of the record at this point.

STATEMENT OF DR. JAMES L. GUMNICK, EXECUTIVE VICE PRESIDENT, GURC, BELLAIRE, TEX.

Dr. GUMNICK. Mr. Chairman and members of the subcommittee, ladies and gentlemen, I wish to address the need for more Federal support for research in fossil fuel development, especially in the area usually described as enhanced oil recovery.

The experience of my organization, GURC, with enhanced oil recovery, goes back to the days preceding the creation of DOE, ERDA, and FEA—in fact, back to the Bureau of Mines.

The experience of our staff represents over 200 years in the petroleum industry reaching back to the 1930's for several of our senior engineer-scientists.

We are a consortium—not for profit—whose members and affiliates include 15 governing universities and 19 energy-related companies. My personal experience covers nearly 30 years in research and technology; 11 in industry, 11 in academia, and 5 in not-for-profit organizations of the consortial type.

This presentation represents the consensus view of our staff and a majority of our constituents.

My reason for being here is that in the pursuit of liquid fuels, there is a need for know-how, for personnel, for resources, and of course, for capital. All of these are especially important in the efficient development of petroleum resources through enhanced oil recovery, and the need is urgent.

When I first joined the University of Houston, in the heart of the oil industry, I asked the dean of engineering of the university why there was no petroleum engineering department. He told me there were too many stops and starts, too much boom and bust. To temper the negative effects of stops and starts on research and development in EOR, I wish to present a case for continuity of effort and continued momentum. This special area of resource recovery is one that can contribute significantly to a reduction of our dependence on foreign oil. It must be used soon or, perhaps, will not be used at all.

May I refresh your memories about enhanced oil recovery? To date, about 450 billion barrels of oil have been discovered in the United States. Of that oil, 150 billion barrels are recoverable—much of it has been recovered—by routine methods. The oil remaining in the ground is 300 billion barrels. Those 300 billion are the target for EOR activity. This amount of oil is about 1,000 times what is projected to be placed in the national strategic petroleum reserve in 1982 and 1983 at a cost of about $6 billion.

In contrast, the proposed budget for enhanced oil recovery in 1983 is $5 million. The relative expenditure per barrel of oil for these two efforts is 1 million times.

No one believes the 300 billion is all recoverable. Expert estimates of what can be produced by currently known EOR methods range from 5 to 80 billion barrels. This spread in estimates represents an uncertainty of 75 billion barrels. If you multiply that by a low price of $20 per barrel, it is an uncertainty of $1.5 trillion. Our Government is planning to contribute $5 million to reducing that uncertainty.

The Energy Research Advisory Board [ERAB] for the Department of Energy, has recognized the need for enhanced oil recovery. In their report titled "Federal Energy R. & D. Priorities," November 1981, they said in part: "ERAB gives enhanced oil recovery a very high priority with respect to technological and economic potential, benefit/cost ratio, urgency, supply security, and health/environmental problems."

Despite this opinion, the recommendation of the group was: "However, the Board recommends that Federal funding, even though relatively small, be decreased. A small Federal expenditure can be justified only for some of the environmental research and to keep current with technical developments in the private sector."

Their reason for such a recommendation was as follows: "This recommendation stems from the considerable evidence that the larger energy companies have the ability, the know-how, and the willingness to engage in sustained, long-term R. & D. Also, the oil companies have the financial strength to commercialize enhanced oil recovery processes without Government-financed demonstrations."

We do not disagree about "the ability, the know-how, and the willingness to engage in sustained, long-term R. & D." of the larger energy companies nor about their financial strength. There are, however, very serious questions about a willingness to engage in high-risk operations without an expectation of commensurate compensation. This is especially true in a market with a declining price structure and an environment that has drained capital estimated to be about $24 billion in 1982 and $21 billion in 1983 via the windfall profits tax. Based on my own experience in industry, this is not an environment conducive to assuming greater risk, and EOR is risky; hence, there is a need for a Federal role to back up the private sector.

As a result of the recent energy crisis preceding our current oil glut, federally-encouraged intitiatives were taken by large and small companies as well as the university community. Those initiatives have created an increasing enthusiasm in investment, resources, and personnel devoted to EOR. Because of the long leadtimes of this activity—and a project does not pay off until 3 to 6 years after being initiated—past Federal financial commitments are only now beginning to be fruitful in increasing our understanding of the applied effectiveness of the processes involved.

This is a poor time to allow a disintegration of the national effort. Unfortunately, because of the competitiveness and proprietary nature of work in this field, the proposed withdrawal of the Government from it will increase the blocks to technology transfer and information exchange. These negative effects will, of course, hit the smaller producers the hardest. These are the people without the financial and research resources needed to independently apply EOR methodologies.

It is worth emphasizing here that proposals being circulated about the disposition of the Bartlesville Energy Technology Center appear equally destructive of the free exchange of knowledge capital, again hurting smaller producers the most. Development of effective mechanisms for this information exchange has been one among the many accomplishments of the Bartlesville group.

For example, funds from the Bartlesville effort in fossil fuels have provided support to 271 personnel in the academic community. From our own studies of EOR research, there have been 94 students graduated and there are 76 more currently active. If this support stops, I wonder where the Government, the universities, and industry will find the researchers for the future?

It will be truly unfortunate if at a time when the Department of Defense is practically begging the academic community to become involved with perceived national problems, another group in the administration should cut off the support for the researchers that recently responded to the requests for involvement in perceived national problems from the Department of Energy. I am reminded of the old vaudeville tune, "How can you believe me when I say I love you when you know I've been a liar all my life?"

There is a serious need for continuity in this type of research as well as for new initiatives. Recall that even the most optimistic forecasts, based on current EOR practices, leave hundreds of billions of barrels in the ground.

There is, moreover, in this area of research and development an urgency that derives from the life cycle of an oil reservoir, and I want to especially highlight this point.

From a study he did titled, "Prospects for Enhanced Recovery of Oil in Texas"—and it is appended to my testimony—Dr. E. L. Claridge, a professor at the University of Houston and a consultant at GURC, has pointed out that unless the rate of enhanced oil recovery is accelerated, according to national trends in oil-well and oil-field abandonment, we will be abandoning 10 billion barrels of oil per year over the next 20 years. This totals 200 billion barrels, and at $20 per barrel represents 4 trillion dollar's worth of oil. Because of the cost and the complexity of reopening closed fields, this oil will become economically inaccessible. The abandonment meter is ticking. That is why we are attempting to reactivate a sense of urgency in the encouragement of increased research, development, and application of EOR.

In conclusion, we strongly recommend restoring and even increasing the budget for enhanced oil recovery and the continued existence of the Bartlesville Energy Technology Center as the integrating agent in that discipline.

The target warrants Federal involvement. The risk warrants Federal involvement. The need warrants Federal involvement. Historical precedent strongly argues for Federal involvement, and the urgency of the task warrants Federal involvement.

I thank you, very much, for the opportunity to present this testimony.

[The prepared statement of Dr. Gumnick follows:]

ENHANCED OIL RECOVERY

A NATIONAL NEED

by Dr. James L. Gumnick

Executive Vice President

GURC

I wish to address the need for more Federal support for research in fossil fuel development especially in the area usually described as enhanced oil recovery.

The experience of my organization, GURC (Appendix I), with enhanced oil recovery goes back to the days preceding the creation of DOE, ERDA, and even FEA--in fact, to the Bureau of Mines. The experience of our staff represents over 200 years in the petroleum industry reaching back to the 30's for several of our senior engineer-scientists. We are a consortium whose members and affiliates include 15 governing universities and 19 energy-related companies. My personal experience covers nearly 30 years in research and technology, 11 in industry, 11 in academia and 5 in not-for-profit organizations. This presentation represents the consensus view of our staff and a majority of our constituents.

My reason for being here is that in the pursuit of liquid fuels, there is a need for know-how, for personnel, for resources, and of course, for capital. All of these are especially important in the efficient development of petroleum resources through enhanced oil recovery and the need is urgent.

When I first joined the University of Houston in the heart of the oil industry, I asked the Dean of Engineering of the University why there was no petroleum engineering department. He told me there were too many stops and starts, too much boom and bust. To temper the negative effects of stops and starts on research and development in EOR, I wish to present a case for continuity of effort and continued momentum. This special area of resource recovery is one that can contribute significantly to a reduction of our dependence on foreign oil. It must be used soon or perhaps will not be used at all.

May I refresh your memories about enhanced oil recovery? To date about 450 billion barrels of oil have been discovered in the United States. Of that oil 150 billion barrels are recoverable by routine methods. The oil remaining in the ground is 300 billion barrels. Those 300 billion barrels are the target for EOR activity. This amount of oil is about 1,000 times what is projected to be placed in the national petroleum reserve in '82 and '83 at a cost of about six billion dollars. In contrast the proposed budget for enhanced oil recovery in '83 is five million dollars. The relative expenditure per barrel of oil for these two efforts is one million times.

No one believes the 300 billion is all recoverable. Expert estimates of what can be produced by currently known EOR methods range from 5 to 80 billion barrels. The spread in estimates, a measure of risk and uncertainty is 75 billion barrels. At $20 per barrel that uncertainty represents 1.5 trillion dollars. Our government is planning to contribute 5 million dollars to reducing that uncertainty.

The Energy Research Advisory Board for the Department of Energy has recognized the need for enhanced oil recovery. In their report titled "Federal Energy R&D Priorities," November 1981 they said in part: "ERAB gives enhanced oil recovery a very high priority with respect to technological and economic potential, benefit/cost ratio, urgency, supply security and health/environmental problems." Despite this opinion, the recommendation of the group was: "However, the Board recommends that Federal funding, even though relatively small, be decreased. A small Federal expenditure can be justified only for some of the environmental research and to keep current with technical developments in the private sector." Their reason for such a recommendation was as follows: "This recommendation stems from the considerable evidence that the larger energy companies have the ability, the know-how and the willingness to engage in sustained long-term R&D. Also, the oil companies have the financial strength to commercialize enhanced oil recovery processes without government financed demonstrations."

We do not disagree about "the ability, the know-how and the willingness to engage in sustained long-term R&D" of the larger energy companies nor about their financial strength. There are, however, very serious questions about a willingness to engage in high risk operations without an expectation of commensurate compensation. This is especially true in a market with a declining price structure and an environment that has drained capital estimated to be about 24 billion dollars in '82 and about 21 billion dollars in '83 via the windfall profits tax. Based on my own industrial experience this is not an environment conducive to assuming greater risk and EOR is risky, hence the need for a Federal role to back up the private sector.

As a result of the recent energy crisis preceding our current oil glut, Federally-encouraged initiatives were taken by large and small companies as well as the university community. Those initiatives have created an increase in enthusiasm, investment, resources, and personnel devoted to EOR. Because of the long lead times of this activity, past Federal financial commitments are only now beginning to be fruitful in increasing our understanding of the applied effectiveness of processes involved. This is a poor time to allow a disintegration of the national effort. Unfortunately because of the competitiveness and proprietary nature of work in this field the proposed withdrawal of the government from it will increase the blocks to technology transfer and information exchange. These negative effects will, of course, hit the smaller producers the hardest. These are the people without the financial and research resources needed to independently apply EOR methodologies.

It is worth emphasizing here that proposals being circulated about the disposition of the Bartlesville Energy Technology Center appear equally destructive of the free exchange of knowledge capital, again hurting smaller producers the most. Development of effective mechanism for this information exchange has been one among the many accomplishments of the Bartlesville group. For example, funds from the Bartlesville effort in fossil fuels have provided support to 271 persnnel in the university community. From our own studies of their support of EOR research there have been graduated 94 students and there are 76 more currently active. If this support stops I wonder where the government, universities, and industry will find the researchers for the future?

It will be truly unfortunate if at a time when the Department of Defense is practically begging the academic community to become involved with perceived national problems another group in the administration should cut off the support for the researchers that recently responded to the requests for involvement in perceived national problems from the Department of Energy. I am reminded of the old vaudeville tune "How can you believe me when I say I love you when you know I've been a liar all my life?" There is a serious need for continuity in this type of research as well as for new initiatives. Recall that even the most optimistic forecasts based on current EOR practices leave hundreds of billions of barrels in the ground.

There is, moreover, in this area of research and development an urgency that derives from the life cycle of an oil reservoir.

From a study he did titled "Prospects for Enhanced Recovery of Oil in Texas" (Appendix II), Dr. E. L. Claridge, a professor at the University of Houston and a consultant at GURC, has pointed out that unless the rate of enhanced oil recovery is accelerated, according to national trends in oil well abandonment, we will be abandoning 10 billion barrels of oil per year over the next 20 years. This totals 200 billion barrels and at $20 per barrel represents 4 trillion dollars worth of oil. Because of the cost and complexity of reopening closed fields this oil will become economically inaccessible. The abandonment meter is ticking. That is why we are attempting to reactivate a sense of urgency in the encouragement of increased research, development, and application of EOR.

In conclusion we strongly recommend restoring and even increasing the budget for enhanced oil recovery and the continued existence of the Bartlesville Energy Technology Center as the integrating agent in that discipline.

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Historical precedent strongly argues to Federal involvement.

The urgency of the task warrants Federal involvement.

Thank you for the opportunity to present my testimony.

Appendix I

GURC

An Independent University-Industry Research Consortium

PURPOSE OF THE CONSORTIUM

Gulf Universities Research Consortium (GURC) was formed and is structured to apply the collective professional manpower of universities in combination with the know-how of industry to research that is beyond the capabilities of single institutions. All such programs are interdisciplinary and normally require special skills and experience in both the science involved and in the areas of application required.

GURC is organized to provide a continuing mechanism, using permanent staff and established policies and procedures whereby:

- university manpower and facilities can be pooled effectively to develop the diversity and experience required to carry out interdisciplinary programs of major scope, and
- these academic scientists are interfaced with the industrial community making use of pertinent data, the state of the art of applicable technology and practice, and an awareness of knowledge needs, and priorities, to create and guide research toward practical goals and produce management-useful results in mission-oriented programs.

THE NATURE OF CONSORTIUM PROGRAMS

The major activities of the Consortium are directed to interdisciplinary R & D programs. Typical characteristics of these programs are that they require:

- a quantity and diversity of talent in the physical sciences, the social sciences, and engineering with levels of experience; or combinations of specialization not available within a single institution;
- equipment and facilities that are not concentrated in a single organization;
- synthesis of geographically-specific data or information (ecological, resources, economic, societal, ...) to accomplish a national, regional, state, or local level appraisal;
- scientific/technical dialogue with industry coupled with an effective data and information exchange necessary for the planning and evaluation of mission-oriented programs;
- centralized program planning, coordination, and technical support; total program synthesis, interpretation, and administration in order that the integrated program produces comprehensive and sound results.

The Consortium's Charter and bylaws state that such programs be concerned with research in the public interest. As a matter of policy

- all data and results produced within a Consortium research program are placed in the public domain following their submittal to the client agency or organization,
- all investigators are encouraged to publish their results in the peer review literature,
- a comprehensive synthesis of total program results is published in the peer review literature when this is appropriate.

Programs for industry are predominantly directed to problems of common interest to a segment of industry and are co-sponsored by the companies comprising that industry by cooperative agreement or via trade associations. The eventual availability of data and publication of results comply with the policies stated above. Proprietary programs are not undertaken.

STRUCTURE

The Consortium is a nonprofit corporation in the State of Texas incorporated in January 1965. Members include universities and industrial organizations. Industrial organizations may also participate as Affiliates. The Consortium was established by universities and, in conformance with its bylaws, remains under university control of all administrative, policy, and procedural matters including programs and products.

The Consortium is, then, a university-industry consortium designed to plan, execute, and coordinate research by the academic community relating to either common industrial needs or the requirements of government. Its policies and procedures carefully ensure that the Consortium remains a creature of the academic community, always under academic direction and control.

GULF UNIVERSITIES RESEARCH CONSORTIUM

1980-81 Members and Affiliates

MEMBERS

University:

- Alabama, University of
- Florida, University of
- Houston, University of
- Louisiana State University
- Miami, University of
- Mississippi State University
- Oklahoma, University of
- Rice University
- Southern Methodist University
- Texas A&M University
- Texas Christian University
- Texas Tech University
- Texas, University of at Austin
- Tulane University
- Universidad Nacional Autonoma de Mexico

Industry:

- Amoco Production Co.
- Hovell Corporation
- Mitchell Energy & Development Corp.
- Texas Eastern Corporation
- Transco Companies, Inc.

AFFILIATES

- Arco Oil and Gas Co.
- Cities Service Oil Co.
- Conoco, Inc.
- Dresser Industries, Inc.
- Exxon Production Research Co.
- Getty Oil Co.
- Gulf Energy & Minerals Co., U.S.
- Mobil Producing Texas & New Mexico, Inc.
- Phillips Petroleum Co.
- Shell Oil Co.
- Sun Production Co.
- Superior Oil Co.
- Union Oil Co. of California
- United Gas Pipe Line Co.

Appendix II

Prospects for Enhanced Recovery of Oil in Texas

by Dr. E. L. Claridge

Professor of Petroleum Engineering
University of Houston
Consultant to GURC

ABSTRACT

Texas contains one-third of all of the oil in the U.S. Consequently, of the thirty to sixty billion barrels of oil expected to be recovered by enhanced oil recovery processes, ten to twenty billion barrels will come from Texas oil fields. The ratio of dolomite and limestone oil reservoirs to sandstone reservoirs is considerably larger in Texas than in the rest of the nation, consequently more emphasis will be placed on CO_2 flooding in Texas than elsewhere since this is the principal enhanced oil recovery process which is applicable to the dolomite and limestone reservoirs. Correspondingly, there will be less emphasis on the use of steam soak and steam drive since most of Texas's oil is not of the high viscosity to which these processes are primarily applicable. There remains a high portion of sandstone reservoirs, most of which are being flooded with water, and many of them approaching the time when an enhanced oil recovery process must be applied if they are not to be plugged and abandoned. The most logical enhanced oil recovery process for these reservoirs is surfactant-polymer flooding. However, this process presently appears to be the most complex and costly of the principal enhanced oil recovery processes, and the economic viability of its application, particularly to the stripper-type situation where the statutory decline rate built into the Windfall Profits Tax Law does not offer significant economic benefit, is presently in doubt. An improved tax treatment for this

large class of enhanced oil recovery prospects is urgently needed. The current incentive program will cause a limited surge of enhanced oil recovery projects of all types, but is not a permanent solution to the problem of oil fields nearing abandonment with two-thirds of the original oil still in them.

Research has apparently solved most of the technical problems with enhanced oil recovery processes other than the strongly deleterious effect of reservoir heterogeneity. This factor typically limits enhanced oil recovery to about one-sixth of the oil remaining. This is the area most in need of improvement through research.

INTRODUCTION

A series of studies have been made by government agencies (1-9) and by industry representatives (10-18) in the attempt to predict ultimate oil recovery from crude oil deposits in the U.S. One approach is statistical (7-10) while the other (1-5, 11-18) estimates enhanced oil (tertiary) recovery and adds it to past production plus estimated further primary and secondary recovery. The statistical approach is exemplified by Figure 1 (based on reference 9), while the additive approach is exemplified by Table 1 (based on reference 12).

In general, the statistical approach has been based on extrapolating historical crude oil discovery and production data as collected and published by the American Petroleum Institute (API) (19). These data reflect only production of oil by primary recovery plus secondary methods (gas cycling, waterflooding, and the limited amount due to steam soak operations in heavy oil reservoirs) since oil production by enhanced oil recovery processes has been only a tiny fraction of annual or cumulative production to date. The primary and secondary recovery efficiencies average about 19% and 14%, respectively, for a total of 33%. Thus, current cumulative production of about 120 billion barrels plus projected additional primary and secondary recovery of 30 billion barrels amount together to one-third of the 450 billion barrels of oil discovered up to now in the U.S. The most recent statistical analysis indicates production peaking in 1970 at 100 billion barrels produced. This is thought to be one-half of the ultimate amount which thus would be 200 billion barrels. It is implied that an additional 150 billion barrels of crude oil will be discovered, and one-third of that amount will be recovered by primary and secondary means.

Optimistic estimates of enhanced oil recovery from the presently-discovered 450 billion barrels range from 30 to 60 billion barrels, while pessimistic estimates range from 5 to 15 billion barrels. If these are increased by a factor of 4/3 to account for enhanced oil from future discoveries, we obtain pessimistic estimates of 7 to 20 billion barrels and optimistic estimates of 40 to 80 billion barrels.

CRITIQUE OF RECOVERY ESTIMATES

It is clear that neither of these methods is complete or very accurate. A major problem in each case is that the analysis is based on the past but attempts to predict the future. This requires that there be no radical change in the factors which have affected the behavior in the past. The past on which the statistical projection is built includes the periodic opening of new oil provinces, in each of which the largest fields were discovered over a few (5-10) years time, after which the average size of new fields declined. It also includes a steady revision upward of the predicted recovery by primary and secondary means. In 1956, when Hubbert made the first analysis of this type, there was relatively little secondary recovery, while at present over half of the remaining oil will be recovered by secondary methods. Within a fixed geographic region, new provinces must become exhausted, and indeed the rate of new discovery of oil has declined since 1964 with the exception of the Prudhoe Bay discovery in 1968. The other new provinces found in the U.S. have been smaller than before. Technological developments (20, 21) may yet make it possible to find a new set of reservoirs at greater depth or in stratigraphic rather than structural traps, within the present geographic boundaries. In fact, such advances will be necessary to find another 150 billion barrels of crude oil as called for by the statistical prediction (within boundaries of the U.S.), since every structure capable of holding 0.1 billion barrels or more which can be detected by present means within explored areas has already been tested by drilling. The 195 fields of this category in the U.S. had 56% of the total reserves.

The problem with enhanced oil recovery estimates is that they are based largely on laboratory data rather than field results. A considerable number of field pilot tests must be completed before reasonably accurate predictions can be made. This difficulty has been recognized in all of the forecasts. A limited amount of field results may cause a considerable swing in enhanced oil predictions. In 1970-75, most predictors assigned the major roles in enhanced oil recovery to surfactant-polymer flooding and thermal recovery methods. Currently the emphasis is on CO_2 flooding plus thermal methods. Actual oil recovery obtained in about 20 field projects of surfactant-polymer flooding is reported (23, 24) to average about 30%, as compared to the value of about 60% assumed by Geffen (12). The latter recovery is about that achieved in the best field tests, hence it is an optimistic estimate and corresponds to a projection of 60 billion barrels of enhanced oil recovery.

ABANDONMENT OF PRESENT RESOURCES

Another difficulty with the enhanced oil recovery estimates is that they have been applied to the total resource base, but unless the application of the three major processes (thermal, CO_2, and surfactant-polymer) is accelerated some of this resource will not be available. Much of the oil unrecoverable by primary and secondary means is in fields which are close to the economic limit of water-oil production ratio. If operations in these fields are shut down, the leases and unit operating agreements will terminate. State laws require abandoned wells to be plugged with cement and surface equipment to be removed, so that no hazard exists. Once these events have taken place, the abandoned fields cannot in general be considered a part of the resource base (there maybe a few exceptional cases where they could be restored). This subject is only occasionally mentioned in the literature (17, 25). Reference (25) gives a plot of the fraction of current production produced from fields discovered prior to a given date (on the horizontal axis). This is reproduced in Figure 2.

Figure 2 illustrates what we all know - that every oil field has a definite life. About 90% have a lifetime less than 40 years. This means that by the year 2000, most of the fields discovered before 1960 will be abandoned or be near the point of abandonment. Fields discovered between 1960 and 1980 will supply less than half of the current production at that time. (This refers to primary and secondary production only). This can be translated into a future history for fields discovered up to 1980, as shown in Figure 3, if enhanced oil recovery methods are not applied. As can be seen, the rate of abandonment of the current target for enhanced oil recovery is very significant, amounting to about ten billion barrels per year over the next twenty years. By that time, about 70 billion barrels of EOR target oil should have been added through new discovery, but three times that much EOR target oil will have been abandoned. If enhanced oil recovery processes are not applied to all of the fields where they are applicable (probably to 1/2 to 2/3) then about 2/3 of the present target will have been abandoned by that time. This represents a loss of 40 billion barrels of the 60 billion barrels optimistically estimated to be recoverable. Evidently, delay in applying enhanced oil recovery methods will tend to make recovery approach present pessimistic estimates. Past estimation approaches have not taken this into account.

This means to me that the industry can't wait for more assurance of technical and economic success from results of pilot tests which require five years or more to conduct and evaluate. Some process must be applied to a large portion of the fields as they approach the economic limit of water-oil ratio.

FINANCIAL ASPECTS

At the same time, prudent managements cannot simply jump off at the deep end if they have no method of reducing the present great risk of economic failure. What is needed is some way of sharing the inordinate financial burden of the enhanced recovery processes with the much lesser burden of primary and secondary oil. The current Incentive Program of the DOE does this. Extra income from the other fields (due to higher price) can be used to carry out an enhanced oil project. The so-called Windfall Profits Tax Act, which actually imposes an excise tax on all oil production, but allows a high statutory decline rate in oil production to which the maximum tax is applied and imposes a much lower tax on the remaining secondary oil as well as on the enhanced recovery, aids the application of enhanced oil recovery processes only to those oil fields which are far from abandonment.

I suggest that the Windfall Profits Tax be quickly repealed, or that the Act be amended to allow a much lower tax on any oil which a company produces so long as the money thus made available is used for qualified enhanced oil projects. The mechanism for certifying such projects already exists in the DOE. This will allow generation of funds not only to cover costs of successful EOR projects but also to cover losses on the inevitable greater number of failures among these projects when they are undertaken at a rapid rate without thorough pilot testing.

TEXAS EOR PROSPECTS

In order to apply the above considerations more specifically to Texas, it is necessary to consider in what ways Texas oil resources differ from those of the entire nation. The major difference is that a higher portion of Texas target oil for enhanced recovery is in low permeability carbonate (principally dolomite) reservoirs. Reference (19) data indicate that for the U.S., the proportion of discovered oil in carbonate reservoirs is 28%, while for Texas the proportion is 46%. Texas has produced slightly more of its primary and secondary reserves than the rest of the nation - 81.3% versus 78.3% - but this is largely due to the presence of the largely unexploited Prudhoe Bay field in the national average. The average ultimate primary plus secondary recovery from Texas carbonate is 30% versus 36% for Texas sandstone reservoirs. The national average is 33.6% for the sandstones and 28.33% for carbonates.

CO_2 PROSPECTS

The dolomite reservoirs of West Texas are particularly suited for CO_2 flooding. Efforts are already underway by many major oil companies to initiate CO_2 flooding in these reservoirs. Natural deposits of CO_2 are being developed in adjacent states of Colorado and New Mexico, in order to supply this need.

Two BETC reports (26, 27) describe the application of screening criteria given in references (1, 2) to select candidate reservoirs for major EOR processes including steam drive, in-situ combustion, CO_2 flooding, surfactant-polymer flooding, and polymer flooding. They select 11 candidates for polymer flooding in the Permian Basin. All but one of these is also suitable for CO_2 flooding. CO_2 is likely to recover more of the oil in place than polymer so it should be preferred when its use is permissible.

Ten of the Permian Basin reservoirs studied are in New Mexico rather than Texas. The original oil in place in the forty Texas reservoirs is 35 billion, and 22 billion will remain as target oil. Of this, about 15 billion barrels is in reservoirs considered suitable for CO_2 or polymer (also for CO_2). None were found suitable for surfactant-polymer or thermal methods.

Of the 50 Gulf Coast Texas reservoirs, seven were found suitable for CO_2 flooding. These contained only 440 million barrels of target oil, however. Twenty-two of these 50 reservoirs had a gas cap and a strong water drive and were not considered suitable for any EOR process. Five more have a strong water drive and were also not considered suitable.

SURFACTANT-POLYMER CANDIDATES

Seven candidates for surfactant-polymer and nine for polymer flooding were found among the 50 Texas Gulf Coast Reservoirs. The polymer candidates look equally suitable for surfactant-polymer and the sum of these categories is 1.1 billion barrels of target oil, out of 4.4 billion barrels in place in these reservoirs. No thermal recovery prospects were found.

SCALE-UP TO DISTRICT SIZE

Original oil in place in the Texas Districts 2, 3, and 4 where these 50 reservoirs are located amount to 30 billion barrels. Assuming that the 50 reservoirs are representative and the same fraction applies to the total, this implies that target oil for surfactant-polymer flooding along the coast is 7.5 billion barrels.

Similarly, the target oil for CO_2 in coastal reservoirs is 3.0 billion barrels.

Applying a ratio for the Permian Basin CO_2 prospects of 22 billion target oil in the 40 Texas reservoirs to 72.5 billion barrels target oil in Districts 7B, 7C, 8, and 8A, we obtain for these districts a total CO_2 target of 45 billion barrels of oil.

The remaining Texas Districts 1, 5, 6, 9, and 10 contain 16.5 billion barrels of oil which will remain after primary and secondary recovery (thus, target oil). Of this, 4.2 billion is in the East Texas field, which is not a good candidate for any of the EOR processes. The remaining 10.3 billion barrels is divided as 4 billion for surfactant-polymer, 2 billion for CO_2, and 4 billion for no process.

In summary, we obtain 50 billion barrels target oil for CO_2 and 11.5 billion barrels for surfactant-polymer.

THERMAL PROSPECTS

Reference (28) indicates that about 30.5 billion barrels of heavy crude oil is present in Texas. Of this, 20.3 billion barrels is between 20 and 25° API, while the other 10.2 billion is less than 20° API. About 10.8 billion of the total is at a depth less than 3000 feet. It will be assumed that the latter amount is in reservoirs suitable for application of thermal methods.

SUMMARY ESTIMATE FOR TEXAS

Applying the recovery factors of Table 1 to the estimated Texas target oil, we obtain the results shown in Table 2. The estimated recovery is 7 to 24 billion barrels. One half of the U.S. CO_2 flooding prospects are in Texas, and about a third each of the surfactant-polymer and thermal candidates.

ECONOMIC ESTIMATES

The CO_2 requirement will be 5 to 10 MSCF per barrel of recovered oil. Hence, 25 to 1500 trillion cubic feet of CO_2 would be required. A maximum supply of 25 to 50 TCF of fresh CO_2 may be expected, hence a high degree of recycle will be required. The amount of CO_2 required to replace the recovered oil as reservoir volume is about 2 1/2 MSCF per stock tank barrel, hence the amount to be left in place of oil is 12 1/2 to

37 1/2 trillion cubic feet. The maximum inventory during the flooding process is about twice that amount (25 to 75 TCF). At an average cost of $2.50 per MSCF this amounts to $60 to $180 billion (in 1980 dollars).

The surfactant requirement is 15 to 20 pounds of active sulfonate per barrel of recovered oil, and none of it is recovered. The amount required in Texas is then 17 to 80 billion pounds (about $12 to $60 billion cost). Cost of polymer and other chemicals will be about equal to these amounts. Fuel cost for thermal recovery will be about 1/2 barrel per barrel of recovered oil, or at $40/bbl a fuel cost of $20 to $100 billion.

Thus, capital or expensed cost of the recovery agents for the estimated enhanced oil recovery will be about $100 billion to $400 billion. Other operating costs will be about $100 billion. The gross value of the recovered oil will be $280 to $960 billion. Currently, gross operating income from Texas oil production is about $40 billion per year. It is clear that long-term financing of very large amounts of money is involved in enhanced oil recovery on a Texas scale.

CONCLUDING REMARKS

It will be noted from Table 1 that with our most optimistic estimates we will recover in total less than 50% of the discovered oil. That is, when we have gone through primary, secondary, and tertiary recovery methods (the latter just now being developed), we foresee having over 300 billion barrels of crude oil worth $40 per barrel in 1980 dollars, in reservoirs to which we have access. Now, $12 trillion is a worthy target for a major technological effort. If we can identify the obstruction in Nature keeping us from it, we will know where to direct research and development and business efforts.

The obstruction in this case is known. It is reservoir heterogeneity. The oil left behind after all of our processes will be in low-permeability regions (layers and areas) while our injected fluids will have travelled through adjacent high-permeability layers and areas. We have made only limited beginnings in attacking this problem. Most of our research efforts have been directed at getting the maximum oil possible out of the high permeability zones. We will approach diminishing returns with these past research directions after 1990. We should begin now to attack the major remaining problem in attaining high recovery of our national and state heritage of crude oil.

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FIGURE 1

CRUDE OIL DISCOVERY, PRODUCTION, AND RESERVES IN THE U.S.A.

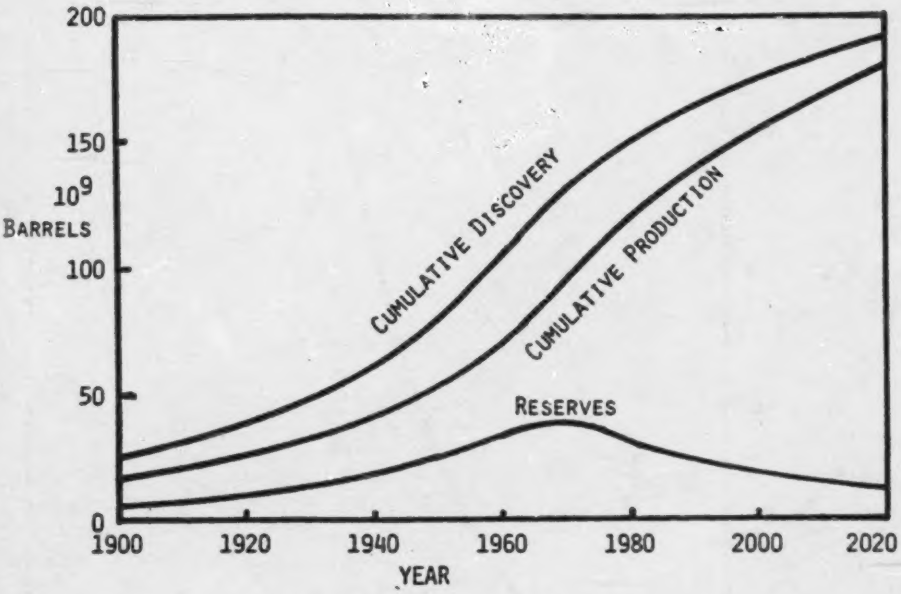

FIGURE 2

DEPLETION OF OIL FIELDS

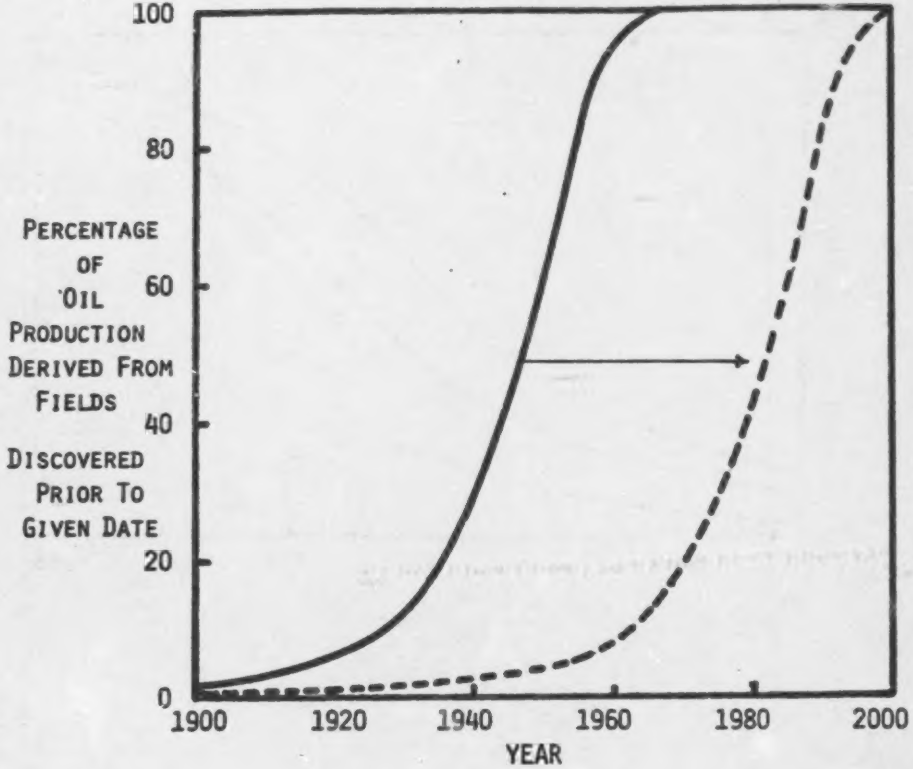

FIGURE 3

FUTURE HISTORY OF PRESENTLY DISCOVERED CRUDE OIL

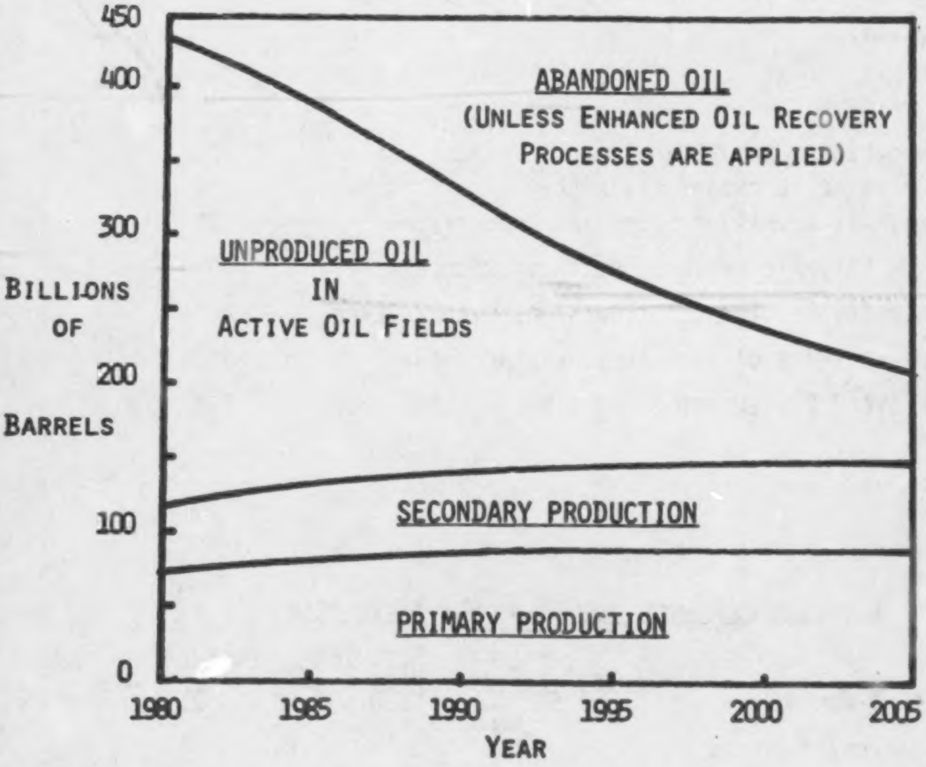

Table 1

Recovery Efficiency of EOR Methods, U.S. Total

	Target Oil, Billion Bbl.	Recov. Eff. High	Recov. Eff. Low	Recovery, 10^9 Bbl High	Recovery, 10^9 Bbl Low
CO_2 Flooding	100	0.30	0.1	30	10
Surfactant-Polymer	40	0.35	0.10	14	4
Thermal	30	0.50	0.10	15	3
Total	170			59	17

	Billions of Barrels
Produced up to 1980:	120
Primary & Secondary Reserves:	30
Ultimate Primary + Secondary Production:	150
High Estimate of Total Ultimate Recovery:	209
Recovery Percentage of Original Oil In Place:	46%
Low Estimate of Total Ultimate Recovery:	167
Recovery Percentage of Original Oil In Place:	37%

Table 2

Recovery Estimates for Texas EOR Target Oil

	Target, 10^9 bbl	Recov. Eff. High	Recov. Eff. Low	Recovery, 10^9 bbl High	Recovery, 10^9 bbl Low
CO_2 Flooding	50	0.30	0.10	15	5
Surfactant-Polymer	11.5	0.35	0.10	4	1
Thermal	10	0.50	0.10	5	1
Total	71.5			24	7

Mr. Fuqua. Thank you very much, Dr. Gumnick.

Mr. Roach, you may proceed.

STATEMENT OF ROBERT L. ROACH, WASHINGTON REPRESENTATIVE, DIRECTOR, SYNTHETIC FUELS PROJECT, ENVIRONMENTAL POLICY CENTER

Mr. Roach. Thank you, Mr. Chairman.

I would like to thank you for the opportunity to appear here today and present the views of the Environmental Policy Center.

I would just like to summarize certain aspects of my testimony.

The budget and policy recommendations of the Reagan administration have reopened the debate over the proper role and appropriate level of Government involvement in research and development. The views of this administration with respect to R. & D. in general and energy R. & D. in particular, reflect a shift toward limited Government involvement, with an accent on basic research that emphasizes long-term, high-risk R. & D., and extensively on health, safety, and environmental issues.

The view of the Environmental Policy Center is that there are some merits in taking such an approach to Federal R. & D. activity if it is not carried to excess.

Innovation and technical change are the keys to this Nation's economic strength. Indeed, these factors may account for as much as one-half to two-thirds of United States economic growth. Yet, many economists believe that U.S. industries do not invest enough of their own resources in fundamental R. & D.

In 1981, industry invested approximately $34 billion in research and development, but most of it was dedicated to enhancing breakthroughs in minor improvements in existing products rather than high-risk investments that lead to scientific breakthroughs.

In recent years, Federal R. & D. expenditures have followed a similar course. More and more Federal dollars were earmarked for improvements of existing technologies, development of marketable products, and the construction and operation of large-scale demonstration facilities.

The problems caused by this are two-fold. No. 1, Federal efforts and dollars are directed from basic research and from smaller, diversified activities, and the Federal Government becomes locked into large, expensive projects which may have eventually been supported by the private sector. Also, such Government involvement may bias the investment decisions of private industry, partly in an effort to benefit from Federal funds and partly out of a fear that Government intervention may accelerate the introduction of new technologies, many businesses will abandon their own research in different areas to concentrate on the areas favored by Government funding. This not only limits the scope of R. & D. activities, but results in the Government bearing a greater share of R. & D. efforts. Indeed, direct subsidies from the Federal Government represent about 35 percent of the average corporate spending in the United States today.

In comparison, in Japan, Government subsidies represent only about 2 percent of corporate R. & D. expenditures. Clearly, there is a need to encourage greater private investment in R. & D. while limiting Federal involvement to basic research.

Nonetheless, although increased R. & D. efforts in the private sector are essential, such a goal may not be immediately achievable in all areas and we must recognize this. Certain sectors, such as regulated industries and small, fragmented industries, may lack the incentive and/or capital to assume R. & D. responsibilities in an adequate and timely fashion. In these areas, the continuation of Federal R. & D. activity may be justified. However, these assessments must be made on a case-by-case basis.

These general views, I believe, may be directly applied to an analysis of the DOE's proposed fossil energy program.

A great deal of concern has been expressed recently about the size of the reductions in this program. Since close to 91 percent of the proposed reductions are derived from the coal program, it is natural to assume that this particular area will be a major focus of efforts to increase funding levels of those recommended by the administration.

Critics of the cuts are legitimately concerned that the already diminished coal program, combined with these new reductions, will severely weaken the coal R. & D. infrastructure and that such sweeping reductions will impair efforts to increase and improve the direct utilization of coal.

However, as this subcommittee considers proposals to increase activities and restore funding levels for this program, it is important to examine the substance of the reductions proposed by the administration. The Environmental Policy Center believes that many of the proposed reductions are justified. The cutbacks reflect a fundamental redirection of the Federal role in R. & D. from the development of near-term technologies and the promotion of commercialization to the concentration on generic R. & D. and basic research.

In the past, the coal program in particular has harbored some very expensive programs of questionable merit. In recent years, great increases in this budget did not represent an increase in basic R. & D. as much as a proliferation of large and expensive demonstration programs which should have been funded by industry.

A great deal of criticism has been put forward that many of the administration's nondefense budget cuts are excessive and in many areas we agree with this. However, this general criticism should not be used as a vehicle to continue the funding of large, multibillion-dollar boondoggles. The approach that this subcommittee takes to the restoration of the coal program funding is critical. To merely maintain activities at last year's levels, or split the difference between last year's authorizations and this year's requests will not ensure a sound, or even reasonable, coal R. & D. program.

We hope that each activity in the program will be viewed very closely and restorations will be made on the merits and potentials of each individual program.

I would just like to take 1 or 2 more minutes to look at some specific areas where we feel that cuts are justified, and also some areas where we feel that increases are indeed necessary.

In examining the large reduction proposed for the coal program, it is important to realize that $115 million or 41 percent of the reductions in new appropriations, and $224.7 million or 57.9 percent of the decrease in real spending authority is located in two activities: coal liquefaction and surface coal gasification.

Indeed, since the formation of the Department of Energy, funding for synthetic fuel technologies has been a major part of the entire fossil energy program. I patched up a table—table 3—in which I tried to review the total amount of DOE funding for synthetic fuels since the DOE was formed in 1978. I did not go back earlier than that because I was not sure that the estimates were consistent as far as funding so I just took it from 1978 through estimates for 1982. If you look at the final column you can see, as a result of total obligation authority and as a result of obligations or outlays, that in most years it is up in the 40- or 50-percent range. In other words, 40 or 50 percent of the authority as well as the obligation of the fossil energy program were in the areas of synthetic fuels activity.

A close examination of the projects proposed for reduction or termination reveals that these are justified. Much of the funding in these activities supported enhancement of proprietary technologies, construction of large-scale demonstration and first-stage commercialization units, and even some unproductive experiments.

The reductions in liquefaction and gasification activities, we believe, represent a fundamentally sound shift to the testing of long-term, high-risk technologies which offer potentially large benefits, as well as collection of data and characterization of pollutants to develop a data base for environmental health programs and mitigation strategies. Indeed, if any areas in coal liquefaction and gasification activities are going to be increased, it should be the latter. In addition to the economic and technological problems, environmental issues represent the greatest obstacles to synthetic fuels development. Failure to characterize critical pollutants and develop mitigation strategies in a timely fashion may come back to haunt the synthetic fuels industry.

It should also be noted that the changes in these programs reflect the proposals made in a November 1981 report of the research and development panel of the Energy Research Advisory Board, entitled "Federal Energy R. & D. Priorities."

Although the panel made no specific funding recommendations, it did advise a curtailment of liquefaction and gasification programs at the large-scale level, with decreased attention to indirect liquefaction and increased concentration of basic R. & D. and environmental issues, with research efforts limited to laboratory and small pilot plants.

I think, Mr. Chairman, it is important to consider that we are dealing with a shrinking pie and important decisions are going to have to be made as to where scarce resources are going to be allocated.

It is the feeling of our organization that much of the money that is in the synthetic fuel area takes away critical dollars from a very important aspect of the fossil energy program and that is the direct utilization of coal. There are many activities in this budget related to that which do merit funding levels higher than those which were proposed.

For example, the ability to develop cleanup systems which can provide for the simultaneous removal of sulfur and nitrogen oxides offer many advantages. Recently, more attention has been given to nitrogen oxide and its relation to the problem of acid rain and it merits further attention.

So, too, do systems which improve particulate removal and advanced systems for the removal of fine respirable particulates. As the direct utilization of coal increases, so will the need for these advanced control systems. The Energy Research Advisory Board has recommended that programs to develop improved emission control technology to reduce sulfur oxide particulates and nitrogen oxide receive both increased funding and a higher priority with the fossil energy program.

We believe that it is also important to increase Federal support of advanced combustion technologies and alternative fuel utilization research. The data generated through these programs may increase the midterm potential for fuel substitution.

In conclusion, Government R. & D. programs should focus on the development of concepts which promise public benefits, but they are a long way from commercial realization. They should not divert Federal dollars to construction and demonstration programs which are the responsibility of the private sector.

If a need occasionally exists to bridge the gap between conceptual breakthroughs and commercialization, incentives, other than direct governmental outlays, should be employed. For example, tax credits would result in far less direct expenditures and provide greater incentives for efficient management than direct Government funding.

Thank you, Mr. Chairman.

[The prepared statement of Mr. Roach follows:]

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Statement of Robert L. Roach
Washington Representative
before the
Subcommittee on Energy Development and Applications
of the
House Committee on Science and Technology
On the Department of Energy's
Fossil Energy Research and Development Program
March 24, 1982

Mr. Chairman, I am Robert Roach, Washington Representative of the Environmental Policy Center. The Center is a non-profit, non-partisan environmental organization which for the past 10 years has worked on national policy issues relating to the use and conservation of our energy, agricultural and water resources.

It is a pleasure to appear before you this morning and offer our views on particular aspects of the Department of Energy's Fossil Energy Research and Development Program.

The budget and policy recommendations of the Reagan Administration have reopened the debate over the proper role and appropriate level of government involvement in research and development (R&D). Clearly, the views of this Administration with respect to R&D in general and energy R&D in particular reflect a shift toward limited government involvement, with an accent on basic research that emphasizes long-term, high-risk R&D and research on health, safety and environmental issues.

The view of the Environmental Policy Center is that the Reagan Administration's approach is not a totally ruinous policy. In fact, there are some merits in taking such an approach to federal R&D activity.

Innovation and technical change are the keys to this nation's economic strength. Indeed, these factors may account for as much as 1/2 to 2/3 of the U.S. economic growth. Yet, many economists believe that U.S. industries do not invest enough of their resources in fundamental R&D. In 1981, industry invested $34 billion in R&D, but most of it was dedicated to minor improvements in existing products, rather than high-risk investments that lead to scientific breakthroughs. In recent years, federal R&D expenditures have followed a similar course. More and more federal dollars were earmarked for improvements of existing technologies, development of marketable products, and the construction and operation of large-scale demonstration facilities. The problems caused by such an approach are two-fold: federal efforts and dollars are directed from basic research and smaller, diversified activities, and the federal government is locked into large, expensive projects which may have eventually been supported by the private sector. There is also less incentive for efficiency and effective management of programs. The overall benefits of such an approach are questionable, at best. Robert Fri, formerly Deputy Administrator of the Energy Research and Development Administration (ERDA), has expressed doubts about the merits of such an approach to federal R&D:

> I'm a lot more skeptical now than I would have been three or four years ago. In the early days of ERDA the strategy was to throw a lot of little marbles on the table -- to start work on lots of little things at low cost. What happened was that most of those little things proceeded to grow and get very expensive. ("The Right Remedy to R&D Lag", Fortune, January 25, 1982).

Concentrating federal R&D money in certain areas may also bias the investment decisions of private industry. Partly in a effort to benefit from federal funds and partly out of fear that government intervention may accelerate the introduction of a new technology, many businesses will abandon their own promising research efforts to concentrate on the areas favored by government funding. This not only limits the scope of R&D activities, but results in the government bearing a greater share of R&D efforts. Indeed, direct subsidies from the federal government represent 35% of the average corporate R&D spending in the U.S. In comparison, in Japan government subsidies represent only two percent of corporate R&D expenditures. Clearly, there is a need to encourage greater private investment in R&D, while limiting federal involvement to basic research. Tools have been made available to effectively implement such a strategy. The 1981 tax bill provides for a 25% tax credit for certain increases in corporate research expenditures and allows for accelerated depreciation of capital equipment used in R&D. Additionally, companies are undertaking, with government encouragement and approval, joint research ventures. This approach reduces R&D costs to individual companies, while increasing the amount of work which may be accomplished in a given period of time.

Nonetheless, although increased R&D efforts in the private sector are essential, such a goal may not be immediately achievable in all areas. As the Energy Research Advisory Board (ERAB) of the Department of Energy (DOE), noted, certain sectors such as regulated industries, and small, fragmented industries may lack the incentive and/or capital to assume R&D responsibilities in an adequate and timely manner. In such areas, continuation of federal R&D activity may be justified. However, such assessments must be made on a case-by-case basis.

These views on federal R&D may be directly applied to an analysis of the DOE's proposed Fossil Energy Research and Development Program.

A great deal of concern has been expressed recently about the size of the reductions in the Fossil Energy (FE) Research and Development budget. Since close to 91% of the proposed reductions are derived from the coal program, it is only natural to assume that this particular program will be the major focus of efforts to increase funding levels above those recommended by the Administration.

Critics of the proposed FE program are legitimately concerned that the already diminished coal program and these new reductions will severely weaken the coal R&D infrastructure. The government would be removed from areas of research which will not be assumed by private industry, and such sweeping reductions will impair efforts to increase and improve the direct utilization of coal.

In the Administration's proposed budget, new appropriations for the coal program are reduced by $366.803 million, or 75%. Real spending authority is decreased by $422.103 million, or 82%.

However, as this Subcommittee considers proposals to increase activities and restore funding levels for this program, it is important to examine the substance of the reductions proposed by the Administration. The Environmental Policy Center believes that many of the proposed reductions are justified. The cutbacks reflect a fundamental re-direction of the federal role in R&D from the development of near term and proprietary technologies and the promotion of commercialization to concentration on generic R&D and basic research.

In the past, the FE budget and the coal program in particular have harbored some very expensive programs of questionable merit. In the late 1970's and 1980, the great increases in this budget did not represent an increase in basic R&D as much as a proliferation of large and expensive demonstration programs and first-stage commercialization efforts which should have been funded by industry.

There has been a great deal of criticism that many of the Administration's non-defense budget cuts are excessive, and in some areas we agree. However, this general criticism should not be used as a vehicle to continue the funding of multi-billion dollar boondoggles. The approach that this subcommittee takes to the restoration of the coal program funding is critical. To merely maintain activities at last year's levels, or split the difference between FY82 authoriziations and FY83 requests will not ensure a sound, or even reasonable coal R&D program.

We urge this Subcommittee to utilize the authorization process to maintain a legitimate R&D program and re-establish the initial purpose of the coal R&D program: the promotion of research to enhance the direct utilization of coal in an efficient and environmentally sound manner. To underscore these points, the remainder of this testimony will review some program activities where the Administraiton's recommendations are justified, and also identify areas where reductions may be too severe.

Coal Liquefaction and Surface Coal Gasification

In examining the large reduction proposal for the coal program, it is important to realize that $115.2 million or 41.7% of the reduction in new appropriations and $224.7 million or 57.9% of the decrease in real spending authority is located in two activities: coal liquefaction and surface coal gasification. Indeed, since the formation of the Department of Energy, funding for synthetic fuels technologies has been a major part of the entire Fossil Energy Program. Federal spending for synthetic fuels and oil shale also greatly outpaced expenditures of the private sector.

A close examination of the projects to be reduced or terminated reveals that the decreases are justified. Much of the funding in these activities supported enhancement of proprietary technologies, construction of large-scale demonstration and first stage commercialization units and some unproductive experiments.

The Solvent Refined Coal I (SRC I) project will be terminated after the project baseline data is submitted to Congress on May 1, 1982. It is a project much larger than needed to demonstrate technical feasibility, yields a poor slate of products, and forces the government to bear 97 % of the costs of a project whose total cost has soared from an original estimate of $685 million to $4.574 billion in 1981. Aside from the technical and market arguments against the project, fiscal reality dictates that the federal government cannot and should not bear such an enormous cost. Over $50 million of the $72.58 million in reductions result from the termination of the 250 TPD Exxon Donor Solvent pilot plant and the 220 TPD H-Coal pilot plant. Both plants have concluded runs utilizing a variety of coals, and have provided enough information to the industrial partners to allow them to make decisions on further scale-up. Ashland Oil has applied to the Synthetic Fuels Corporation for assistance to construct a commercial scale facility, while Exxon has decided the economics of the EDS process are not yet attractive enough. Moreover, these facilities will not vanish. They can and should be made available to other private firms who wish to rent them and operate them at their own expense.

Decreases in the coal liquefaction budget are also reflective of the diminished need for government support for indirect liquefaction processes. The private sector is supporting extensive efforts in this area. Proof of this activity may be found in the number of projects employing indirect liquefaction technologies which applied to the SFC and are candidates for subsidies. Four of the eleven projects employ indirect liquefaction processes.

Reductions in the Surface Coal Gasification Area reflect a conclusion of some federal funded projects and an increase in gasification programs outside DOE. The projects at the Bi-Gas and HY-Gas process development units (PDU) were concluded. Exxon assumed control of the development of its catalytic coal gasification process. The Westinghouse fluidized bed gasifier no longer needs federal assistance. Westinghouse will employ its gasifier in a commercial demonstration project in South Africa, and is planning a 10,500 BPD facility in Pennsylvania and will apply for SFC subsidization.

Clearly, the reductions in the liquefaction and gasification activities represent a fundamentally sound shift in federal focus from the development, construction and operation of facilities to encourage commercialization to the testing of long-term, high-risk technologies which offer potentially large benefits, collection of a data base and the characterization of pollutants to develop a data base for environmental and health programs and mitigation strategies. If any project in coal liquefaction and gasification is to be increased, it should be the latter. In addition to economic and technological problems, environmental issues represent the greatest obstacles to synthetic fuels development. Failure to characterize critical pollutants and develop mitigation strategies in a timely fashion may come back to haunt the synthetic fuels industry.

It should also be noted that the changes in these DOE programs reflect the proposals made in a November 1981 report of the Research and Development Panel of the Energy Research Advisory Board, entitled Federal Energy R&D Priorities. Although the panel made no specific funding recommendations, it advised a curtailment of the liquefaction and gasification programs, with decreased attention to indirect liquefaction and large pilot and demonstration plants. Instead, the Board recommended that efforts in these areas concentrate on basic R&D and environmental issues, with research efforts limited to laboratory and small pilot plants.

Recommended Increases

There are activities in this budget, however, which merit funding levels higher than those which were proposed. Certain long term technologies offer advantages which are not easily factored into traditional cost-benefit analyses. Combined flue gas cleanup technology is such an example. Traditionally, separate approaches have been taken to control the major pollutants from coal combustion: SOx, particulates and NOx. SOx and particulates have received the most attention. Recently, however, more attention has been given to NOx as a contributor to the problems of acid rain and reduced visibility. Therefore, the ability to develop a cleanup system which can provide for the simultaneous removal of SOx and NOx offers many advantages.

So, too, do systems which improve particulate removal, and advanced systems for the removal of fine respirable particulates. As the direct utilization of coal increases, so will the need for these advanced control systems. The Energy Research Advisory Board has recommended that programs to develop improved emission control technology to reduce SOx, particulates and NOx receive both increased funding and a higher priority.

It is also important to increase federal support of advanced combustion technologies and alternative fuel utilization research. The data generated through these research programs may increase the mid-term potential for fuel substitution.

In conclusion, government R&D programs should focus on the development of concepts which promise public benefits, but are a long way from commercial realization. They should not divert federal dollars to construction and demonstration programs which are the responsibility of the private sector. If a need occasionally exists to bridge the gap betwen conceptual breakthroughs and commercialization, other governmental incentives should be employed. Tax credits, for example, would result in far less direct expenditures and provide greater incentives for efficient management than direct government funding.

TABLE 1

FY 82 - FY 83 CHANGES IN APPROPRIATIONS - COAL PROGRAM
(in millions of dollars)

	83 Request	82 Appropriation	Difference	Percent of Total Cuts
Control Technology and Coal Preparation	8.6	25.152	16.552	5.997
Advanced Research and Technology Development	19.8	56.256	36.456	13.208
Coal Liquefaction	26.2	98.784	72.584	26.298
Combustion Systems	6.5	30.544	24.044	8.711
Fuel Cells	9.5	34.464	24.964	9.044
Heat Engines	0	15.408	15.408	5.582
Underground Coal Gasification	.7	8.256	7.556	2.737
MHD	0	21.840	21.840	7.912
Mining R & D	1.0	10.992	9.992	3.620
Surface Coal Gasification	10.5	53.088	42.588	15.430
Program Direction	8.0	12.019	4.019	1.456
	90.8	366.803	276.003	99.995[1]

[1]Number does not equal 100% due to rounding.

Liquefaction and Surface Coal Gasification Reductions

	FY 82-83	% of Total Cuts
Liquefaction	72.584	26.29
Gasification	42.588	15.430
Total	115.172	41.728
Total reductions in all other coal program activities	160.831	58.271

Table 2

FY 82 - FY 83 CHANGES IN BUDGET AUTHORITY - COAL PROGRAMS
(in millions of dollars)

	83 Request	82 Total BA (82 Appropriations plus deferrals)	Difference	% of Total Cuts
Control Technology and Coal Preparation	8.600	25.152	16.552	3.92
Advanced Research and Technology Development	19.800	56.256	36.456	8.93
Coal Liquefaction	26.200	228.384	202.184	47.89
Combustion Systems	6.500	41.044	34.544	8.19
Fuel Cells	9.500	34.464	24.964	5.91
Heat Engines	0	15.408	15.408	3.65
Underground Coal Gasification	.700	8.256	7.556	1.79
MHD	0	27.840	27.840	6.595
Mining R & D	1.00	10.992	9.992	2.367
Surface Coal Gasification	10.500	53.088	42.588	10.08
Program Direction	8.0	12.019	4.019	.95
	90.08	512.903	422.103	99.962[a]

[a] Number does not equal 100% due to rounding.

Liquefaction and Surface Coal Gasification Reductions

	FY 82 - 83	% of Total Cuts
Liquefaction	202.184	47.89
Gasification	42.588	10.08
Total	244.772	57.98
Total reductions in all other coal program activities	177.331	42.01

TABLE 3

SYNFUELS EXPENDITURES AS A PERCENTAGE OF THE FOSSIL ENERGY BUDGET

(all figures in millions of dollars)

		Coal Liquefaction	Surface Gasification	In Situ Gasification	Oil Shale	Total Synfuels	Total FE Program	Synfuels as % of total FE Program
1978								
Total Obligation Authority (TOA)		199.8	194.8	13.0	29.0	436.6	894.0	48.8%
Obligations (OBS)		109.8	115.0	12.8	28.7	266.3	682.2	39.0%
1979	TOA	223.1	181.7	15.2	49.9	469.9	984.3	47.7%
	OBS	139.3	71.8	14.9	34.3	260.3	745.2	34.9%
1980	TOA	332.7	221.4	10.3	43.8	608.2	1094.7(1)	55.6%
	OBS	246.7	141.9	10.2	39.5	438.3	894.3 (1)	49.0%
1981	TOA	363.7	149.7	10.1	37.4	560.9	964.0 (2)	58.2%
	OBS	279.3	131.7	9.4	35.3	455.7	833.9 (2)	54.6
1982	TOA	228.4	53.1	8.3	19.2	309.0	566	54.6%
	OBS	-	-	-	-	-	-	-

1. Does not include $5,518 million in TOA and $101.4 million in OBS for Alternative Fuels Program.

2. Does not include $5, 116.6 million in TOA and $504.6 million in OBS for Alternative Fuels Program.

3. Source: DOE Sunset Review, February 1982 (DOE/PE-0040; vol. 3)

Mr. **Fuqua**. Thank you, Mr. Roach.

Mrs. **Schneider**. Mr. Chairman, I wonder if I might ask for unanimous consent to submit a few questions in writing?

Mr. **Fuqua**. Most certainly.

Mrs. **Schneider**. Thank you. I have another appointment and I am losing my voice.

Mr. **Fuqua**. Mr. Anthony. Your prepared statement will be entered into the record.

STATEMENT OF TOBIAS ANTHONY, DIRECTOR, BUSINESS DEVELOPMENT AND WASHINGTON AFFAIRS, RESEARCH COTTRELL

Mr. **Anthony**. Thank you, Mr. Chairman and members of the Subcommittee on Energy Development and Applications.

My name is Tobias Anthony. I am Vice President of Business Development and Washington Affairs for Research Cottrell. I feel privileged once again to appear before your subcommittee to discuss the R. & D. needed to burn coal cleanly, or, to explain it the way Bob did, direct coal utilization.

I should like to summarize the testimony I have submitted to you to confine my oral statement to 10 minutes.

For years Research Cottrell has been promoting the principle of basing our economy on coal in order to achieve energy independence. For almost 70 years our firm and several of our competitors have invested our own funds exclusively in the research needed to burn coal cleanly.

All of the major pollution-control equipment used to clean up flue gases today, emitted from coal-fired boilers, were developed by the private sector before the Clean Air Act was passed in 1970.

During the 1970's the Federal Government began to invest in R. & D.; first, to assure that the equipment could be upgraded to meet new and more stringent emission limits called for by the Clean Air Act of 1970 and then the more restricted limits in the Clean Air Act of 1977.

Since our market today is now driven by political rather than engineering demands: Since the political demands are unpredictable, and since the cost of R. & D. dictates extraordinarily long payback periods, I sincerely doubt the private sector will ever invest substantial internal funds for long-range R. & D. to burn coal cleanly.

I would like to amplify that point, Mr. Chairman. Interest costs being what they are today, payback periods for long-range R. & D. are usually in the 10 to 13 year timeframe. However, the Clean Air Act is amended every 4 years and future amendments could very well obsolete equipment that would be under development and obsolete it very rapidly. What I am indicating is that it is perfectly comfortable for private firms to invest in R. & D. when the market is predictable but they are most comfortable in investing over the 3 to 5 year timeframe and conditions being what they are today we do not have that kind of timeframe.

So, the question becomes: Who will develop the next generation of control technology? That future technology is essential for two reasons. Post combustion clean-up technology—which is the kind of equipment that my company supplies, will be obsoleted by more cost-effective equipment designed to clean up coal during combustion or precombustion phases. Second, the emerging coal export market offers this Nation another chance to export technology to industrial and Third World countries converting from dependency on oil to coal.

Between now and the end of this century someone will commercialize cleaner ways to burn coal and eventually a super-clean coal. But costs of capital being what they are, it will be a foreign firm where partnerships between Government and industry are routine.

To be competitive with world oil prices, coal would have to be cleaned to quality levels comparable to fuel oil. Techniques for producing and burning alternative fuel forms, such as slurries of coal-water, coal-oil, and coal-alcohol mixtures, for example, could reduce this cost by eliminating coal handling and storage requirements and substantially lowering the environmental control portion of these costs to levels associated with oil firing. Or, perhaps it will be pressurized-fluidized bed combustors with gas turbines, or coal gasification combined cycle systems utilizing turbines or fuel cells that will satisfy a share of this need.

It will take several years of R. & D. before one or more of these technologies emerges.

Importers of coal will be forced by exogenous factors to burn it cleanly and economically. What better products could this Nation offer as a stimulus to increased sales of all ranks of U.S. coal than a complete spectrum of new utilization technologies permitting use of coal at minimum cost and with minimal environmental intrusion.

This Nation could never remain preeminent in solely the export of bulk material. Our success in agriculture resulted from supplying the world's every facet of technology needed to maintain, operate, and even develop its own independent systems. Because of our extensive resources of coal, it is to our advantage to prevail in the export markets and that position can only be achieved and sustained in conjunction with selling technology.

But, that technology must be developed, tested, and commercialized here, first. The DOE's fossil energy coal utilization program is on target for participation by the Federal Government, but $8.6 million for control and coal preparation for fiscal year 1983 and $6.5 million for combustion systems appropriations is ludicrous. The combined appropriations for fiscal year 1981 were $78 million and for fiscal 1982 they were $55.6 million.

I suggest you freeze appropriations at the level of $55.6 million—the fiscal 1982 number—as a minimum and evaluate results for fiscal 1984.

I apologize, Mr. Chairman, for the length of this "brief" summary, but this is the best I could do to deal with this very important subject.

Thank you.

[The prepared statement of Mr. Anthony follows:]

STATEMENT OF
TOBIAS ANTHONY
VICE PRESIDENT
BUSINESS DEVELOPMENT
AND
WASHINGTON AFFAIRS
RESEARCH-COTTRELL INC

Good Morning, Mr Chairman and members of the House Subcommittee on Energy Development and Applications. I am privileged, once again,to appear before your subcommittee to discuss R&D needed to burn coal cleanly.

Twice before, I have testified before this subcommittee. On both occasions, I suggested EPA be free to work on new threats to coal combustion in support of new standards and allow DOE to develop component and systems technology. I am happy to report that DOE reorganized around the suggestion I made to you, namely, putting one person in charge of post-combustion, combustion, and pre-combustion technology development at a lower than the Assistant Secretary level. Now, trade-offs can be made to develop the most cost-effective future technology to assure that this nation by being coal dependent will be free of dependence on foreign oil.

That bit of history aside, I should like now to address questions put to me by your staff.

The most important statement I can make today is that I sincerely doubt the private sector will ever invest substantial internal funds for long-range R&D needed to burn coal cleanly.

The reasons are clear. First, it takes more than ten years to recover an investment in fundamental R&D, based on cost of capital, or interest rates, in the range of 10-12%. Since the Clean Air Act can change every four years, technology becomes obsolete before the payback period is realized. For example, for more than fifty years the weighted wire precipitator was a basic technology used to control particulates from coal-fired utility boilers when reduction of 80-85% were called for. When the 1977 Clean Air Act called for 99% reductions, this type of precipitator was obsoleted in favor of rigid discharge electrodes and fabric filters. Now, it is one thing to track a customer's spending attitude in order to anticipate the need for changing technologies and an entirely different matter when an Administrator of the EPA can change requirements in a morning press conference.

A more glaring example of how political decisions can distort sound long-range planning involves Mr. Costle, former Administrator of the EPA, in a decision regarding percent reduction of sulfur oxides after combustion of low sulfur coal. From 1970 to 1979, wet limestone scrubbers were used to reduce sulfur dioxide in flue gases. When the New Source Performance Standards for utility boilers was amended in 1979, he allowed 70% reduction of sulfur dioxide from burning low sulfur coal, but called for 90% reduction when high sulfur coals are to be burned. His stated purpose was to give spray-dryers a chance to compete with limestone scrubbers. That advantage worked only too well since limestone scrubbers were eliminated from the low sulfur coal marketplace. Perhaps, the most damaging action taken by the federal government was to initiate the Synfuels Express in 1980 only to see it derailed in 1981.

We must understand the full impact of the disillusionment with government dominated markets. All of the technologies that are now in use to control air pollution in the post-combustion phase were developed in the private sector without substantial federal funds. Electrostatic precipitators were invented seventy years ago, scrubbers were developed more than twenty years ago, as were fabric filters and spray-dryers. Federal money has been limited, essentially, to demonstration, test, and evaluation of these technologies.

The question is: who will develop the next more important generation of control technology?

I believe the private sector will limit investment to product improvement, which typically involves payback periods of three to five years, but not major breakthroughs. In effect, the private sector will be willing to assume three to five year risk periods, or roughly the cycle of change of the Clean Air Act. That means the federal government, if it expects to achieve a coal dependency policy that will be cost competitive with imported oil, will have to finance the first five years of development of major breakthroughs. Essentially, the period from test tube to proof-of-concept or pilot.

Future cost breakthroughs must occur in the combustion and pre-combustion phases. It will be more cost effective to eliminate pollution in those phases. I believe only one more technology is needed in the post-combustion phase and that is aimed at combining sulfur oxide, particulate, and NO_x removal.

The lead responsibility for developing those breakthroughs must lie with the Department of Energy. The control technology, coal preparation, and combustion systems programs in the Office of Fossil Energy Research and Development are sound, modest, and worthy of continuation. I suggest rather than gut these programs as would the FY 1983 request by the Administration, that FY 1983 appropriations be frozen at the FY 1982 level. This would amount to a 30% reduction from FY 1981 levels.

This action would free EPA to pursue research needed to fulfill the obligations of statutes such as the Clean Air Act. While it is clear, responsibilities lie with EPA for monitoring, information transfer and health research, it is not clear where their responsibility lies in control technology development. Should EPA be funding development of LIMB, which is aimed at controlling sulfur dioxide and NO_x in a combustion stage or should they work on matters such as fine particulate control? It depends upon the assumption we make. If we assume the EPA is in the business of funding development of control technologies designed for ever stricter emission limits, then one could argue they should be working on LIMB for the reasons of tighter NO_x controls. On the other hand, the responsibility would be better handled by DOE, because they are in a position to decide whether LIMB will be more effective than say combined post-combustion control or catalytic conversion. To get around conflict of judgement, EPA and DOE have collaborated in interagency programming and decision-making.

For reasons of efficient utilization of resources, Congress must decide that EPA limit itself exclusively to the development of technologies in support of new source performance standards called for by a statute. DOE, on the other hand, should be working to develop technologies that would allow us to burn coal cleanly and effectively as part of a national program of energy independence through coal dependence.

The Department of Energy's justification for Federal support of advanced coal utilization R&D has been well founded. Their program will provide more cost-effective and environmentally acceptable means for coal usage from new boiler capacity that will be built by the end of this century. Currently, there exists about 95,000 MW of utility oil-fired capacity, 30,000 MW which involve coal designed boilers that are now being fired with oil and 60,000 MW which were designed specifically for and are now being fired with oil The combined daily oil consumption for this capacity is currently about 1.2 million barrels per day. In the industrial sector, about the same amount, slightly over 1.3 million bbls/day of oil are now consumed in boilers to raise steam, and in furnaces to supply process heat.

As far as new coal-fired capacity, it now costs around 200 to 400 percent more to build a coal-fired plant than an oil-fired unit. About 25 percent of this cost differential is tied up in coal handling and storage and an addition 25% in environmental control equipment. The remainder is due to the larger and more complex boilers required for coal along with the additional ancillary equipment. So with current coal utilization technology, the utilities alone, for example, will have to pay about a $28 billion premium to burn coal in the new 92,000 MW of capacity they project for the 1990 to 2000 timeperiod.

The availability of new technologies such as the nethods to clean coal to quality levels comparable to fuel oil and techniques for producing and burning alternate coal fuel forms such as slurries or coal-water, coal-oil and coal-alcohol mixtures could have a major impact on this cost by eliminating coal handling and storage requirements and substantially reducing the environmental portion of these costs to levels associated with oil firing. In addition to these advanced coal preparation techniques, new power systems such as coal combustion combined cycles involving pressurized-fluidized bed combustors with gas turbines, or coal gasification combined cycle systems utilizing turbines or fuel cells can be of particular importance to the new utility capacity projected for construction during the last decade of this century. Moreover by developing these more cost effective and environmentally superior technologies to enable the utilities to expand the use of coal, some of these same technologies can also make the use of coal more attractive in smaller size boilers and furnaces and significantly enhance the increased industrial use of coal. With a reasonable level of economic recovery in this country, over 100 Quads coal usage, or the demand for over four billion tons can be expected to develop in the industrial sector over this same ten year period. These advanced technologies can help U.S. industry in this major shift in fuel as well as the utilities by providing a more cost effective and environmentally acceptable means of using coal.

So it appears that the original justifications for DOE's Fossil Energy coal utilization program were quite valid, and remain so today. It is particularly tragic that the FY 1983 budget request of just over $100 million for all of our national Fossil Energy R&D activities relegates coal utilization research to almost a meaningless level. About $25 million are requested for utilization compared to the $138 million appropriated by this Congress in FY 1982. The reason this is tragic is that there appears to now be a national need materializing for these utilization technologies which could be of even greater importance to this country than the importance we have previously associated only with our domestic energy problems.

That importance is centered in the major coal export market that this Administration supports and the substantial economic effect that it can have on our balance of trade through the rest of this century. The market is the developing world demand for coal and the world need for technologies to more effectively use it.

It is projected that by 1995, the total free world market excluding the U.S.A. will increase its coal consumption by 670 million tons. At an approximate delivered price of $60/T (equivalent to about $2.5/million BTU), this increase represents an additional market of around $40 billion, with about 50% of that going to coal producers and the other 50% to distributors. If the U.S. retains a 60% share of this market, as we have in Europe, our coal producers and distributors could enjoy about $24 billion worth of new export business over these next 13 years solely for the export of the bulk coal alone.

U.S. development of new coal utilization technologies could add significantly to this volume of export business by also positioning the U.S. as the world's major supplier of coal utilization equipment in addition to coal itself. These technologies might also prove to be instrumental in maintaining our export market as competition increases for indigenous supplies of U.S. low sulfur coal and we begin to move more and more of our lower quality coals from the midwest into this marketplace.

Coal combustion emissions (NO_x, SO_x, particualtes) are currently not regulated in most countries of the world as they are in North America. Part of this reason is that not much coal is currently used, and where it is, the needs are for the most part satisfied by low sulfur coal. With the levels of world increase we are projecting, it is safe to assume that significant amounts of U.S. high sulfur coal and lower rank lignitic and subbituminous coals will eventually be competing for a major portion of this global demand, and emissions in most of the countries using coal will rapidly become regulated.

What better products could this country offer as a stimulus to increased sales of all ranks of U.S. coal than a complete spectrum of new utilization technologies which would permit conformance to these emerging emission regulations at minimum cost and with minimal environmental intrusion. We have never, as a country, developed an export market that has been successful solely upon the value of a bulk natural resource for sale. Our successes are in aircraft, computers, oil exploration, and agriculture to mention a few. Here we supply the world's every facet of technology needed to maintain, operate, and even develop their own independent systems. We should do the same for coal.

Our coal export policy must recognize the total value of the U.S. supplying not only coal, but everything any country would ever need to effectively utilize it. As an example, high ash coals from China, South America and other countries will eventually be competing in the market as their production base develops. If future world market decisions are to purchase these coals, mined at cheap labor costs and nearby to minimize transportation costs, the U.S. should be in a position to supply fluidized bed technology or advanced coal cleaning technology for their most cost effective use. Likewise, we in the U.S. as the dominate country in the world with regard to coal reserves and coal utilization know-how should commit ourselves to be the ones to sell all ranges of technology for upgrading coal quality, altering its solid condition to more acceptable fuel forms and providing the means to cleanly burn all grades of inferior coal directly in a cost effective and environmentally acceptable manner.

In the 1990's for instance, this country should be building coal-water mixture preparation facilities in the Soviet Union or other countries as International Harvester is now doing in the USSR for tractor production. The U.S. should not limit its sights for supplying this new world market to only the bulk commodity of coal itself.

Now, for this coal utlization technology to be developed to a sufficient state to be commercial by the 1990's will depend upon a continued R&D funding commitment by the Federal government over the next several years. Private industry, although relieved of many financial burdens by this Administration's actions and thus better able to fund this type of development, will not be able to take up the slack created by the major cutback in Fossil Energy funding and fully develop these technologies on their own in time for this emerging 1990-2000 market. We fully agree with the R&D policy of limiting the Federal role to longer-range, high-risk activities. Also, with the knowledge that our competitors are not now receiving unfair advantages in the financing of demonstration activities by the Federal government, we feel very comfortable with all the financial risks that these demonstrations can involve.

However, that is exactly where our available research funds will be going over these next few years -- into the nearer term,final development and demonstration efforts. We must depend upon the Federal government to maintain a responsible level of funding for these longer range coal utilization activities if they are to ever materialize. When that technology base reaches a point where we in industry can with some confidence, fit it into our overall business plans for profitable final development and commercialization, you can be assured that no further Federal efforts will be needed. This type of Federal-private sector partnership can eventually work for this country as effectively as it has in Japan and Germany. We are now competing in the world market not only with industrial giants, but also with foreign industrial-governmental conglomerates.

Thank you for the opportunity to express these views, and I will be happy to answer any questions you might have.

Mr. Fuqua. Thank you, very much, Mr. Anthony.

Dr. Yancik, your prepared testimony will be inserted into the record.

STATEMENT OF DR. JOSEPH J. YANCIK, VICE PRESIDENT, RESEARCH, NATIONAL COAL ASSOCIATION

Dr. Yancik. Good morning, Mr. Chairman.

I am Joseph J. Yancik, Vice President for Research at the National Coal Association.

Thank you, Mr. Chairman, for honoring our request to appear before you and to present the views of the association on the proposed fiscal 1983 budget for fossil energy.

Before I do so, I want to reaffirm the association's board of directors' unqualified support for the President's comprehensive program to reduce Federal spending and restore stability and strength to the U.S. economy. This reaffirmation is rooted in a March 1981 letter to the President, announcing support for his economic recovery program goals, even though, as the letter made clear, the coal industry's leaders fully expect it to have an adverse affect on many ongoing research projects. If I might make a parenthetical comment, this is indeed what has happened.

Last week, NCA's board of directors reviewed the progress of the President's economic recovery program. They also considered the President's fiscal year 1983 budget request for coal R. & D. I can report that our board members remain unanimous in their continuing support of the President's efforts to reduce the Federal budget.

However, at the same time, the coal industry's leaders wish Congress to know their firm belief that budget limitations must be balanced amongst all energy options. Only a balanced energy research program can be in the Nation's best interests. The economic viability and environmentally acceptable use of all of our energy resources in a wise and efficient manner must be preserved through adequate research and development. Our directors also took note of statements by the committee's distinguished chairman, and other members of the committee, who expressed similar views. We commend you, Mr. Chairman, and we support your commitment to seek a more balanced effort by the Federal Government in energy technology developments.

The application of the administration's research philosophy of redirecting the Federal role to long-term, high-risk activities with substantial potential payoffs is said to be the basis upon which DOE justifies a drastic reduction in fossil fuel research and development funds. The fiscal year 1983 budget reductions relegate coal research to an insignificant 1 percent of the total energy R. & D. budget.

In the Department's view, the rationale for such an imbalance is explained by their saying that they phased out only those projects that have inhibited or distorted energy producer or user behavior in the marketplace.

It is difficult, if not impossible, to accept that the Department of Energy used common criteria to define market disturbance in an equal or consistent manner to all energy fuel research programs.

We believe that if they had done so properly, the fiscal year 1983 research budget would have shown a much better balance.

Apparently, two meaningful criteria were not used. First, an analysis of our Nation's energy supply options relative to the distribution and efficient use of domestic energy resources; and second, an assessment of the progress, accomplishments and benefits of ongoing energy R. & D. programs to the well-being of the American consumer and taxpayer.

We firmly believe that a reevaluation of DOE's budget decisions are needed, for this Nation can ill afford to turn its back on coal or any viable source of energy on the basis of perceived philosophical views of what does or does not distort the marketplace.

We believe the Federal Government should allocate in a responsible manner among all viable energy fuels whatever research funds are available and that this allocation should be consistent with its role to preserve present as well as future energy options.

Since 1932, the coal industry has supported modest research efforts on a continuing basis at its Bituminous Coal Research, Inc., laboratory. Currently, many of the larger coal producers have their own research facilities or they sponsor research projects at institutes and universities. Much of the industry's ongoing research efforts of necessity have been limited to problem solving rather than the development of new technologies for mining and using coal.

There have always been, and will be in the foreseeable future, limitations to the amount of research dollars that a company can afford to invest. There are two primary reasons:

First, coal markets are highly price competitive. Most small and medium size producers do not have the capabilities to conduct research. In practical terms, this establishes a limit to optional costs, like research expenses, that larger companies can incur and still compete in the marketplace.

Second, coal mining or use techniques or technologies are rarely patentable, nor can they be kept proprietary and protected to prevent other producers or users from reaping economic benefits. Under these circumstances, it is difficult for individual coal companies to commit large sums of money to expensive or high-risk, multiyear research projects.

The coal industry has a long history in evolutionary development of improved mining equipment and methods. In this respect, the industry has relied on the marketplace to introduce these new products and technologies. We are, therefore, supportive of the new Federal research philosophy of having the private sector fund the near-term R. & D. It should be recognized, however, that many ongoing Federal research projects in coal mining and in coal use defy simple characterizations as to their stage of development. Near-term objectives should not be confused with the readiness of the technology for commercialization.

We believe it is unrealistic for the DOE to expect the private sector to pick up many of these unfinished research projects which will be terminated under the present fiscal year 1983 budget request. For some areas, research progress has not yet reached milestones which would permit the private sector to adequately assess the technical risks. And, for those few projects which have reached precommercialization status and for which the marketplace suggests a viable business opportunity, the available decisionmaking time on the part of the private sector may be too brief. It takes time for a company to conduct the necessary technology and market assessments and secure corporate capital commitments. Under DOE's timetable Federal funding will soon be terminated and dedicated research facilities closed and the technical teams disbanded.

It is fallacious for DOE to suggest that failure of the private sector to pick up projects which will be terminated by the fiscal year 1983 budget is proof that the Federal effort was misplaced in the first place. Since current market conditions affect the timing of new corporate ventures, it is unrealistic to conclude that the industry's reticence to move forward in these difficult economic times is an indictment of the usefulness of the technologies scheduled for termination. In the past, many Federal coal research accomplishments have been picked up in due time by industry and developed into commercial products or uses.

The specific coal technologies which the National Coal Association has singled out in past presentations to this committee as warranting Federal support because they complement the private sector research efforts, are as follows:

One, the innovative extraction technologies which significantly reduce health and safety hazards to mine workers, but yet offer improved productivity;

Two, coal cleaning and processing systems which reduce sulfur content, ash minerals and other undesirable trace elements in the coal prior to burning;

Three, combustion systems that produce environmentally benign solid and gaseous waste products; and

Four, synfuel products using conversion processes which can be environmentally controlled to protect the health and welfare of the workers, and also the local community.

The continuing need for research in these technologies, which have been the cornerstone of DOE's fossil energy program, must be reassessed more carefully than what is apparent from the justification submitted by the Department of Energy in its request for fiscal year 1983 coal research funds.

In conclusion, an unbalanced energy research program is not in the Nation's best interests. A viable Federal coal research program is the key to wise and efficient use of the Nation's largest source of energy fuels.

Thank you.

[The prepared statement of Dr. Yancik follows:]

Statement

of

Joseph J. Yancik
Vice President
National Coal Association

Mr. Chairman and Members of the Subcommittee:

I am Joseph J. Yancik, Vice President for Research at the National Coal Association. The association represents the country's major underground and surface coal producers, equipment manufacturers and suppliers, consultants, coal sellers and other organizations providing services to the coal industry. I thank you for this opportunity to present the coal industry's views on the Department of Energy's proposed FY 1983 budget for fossil energy research and development.

Before I do so, I want to reaffirm the association's Board of Directors' unqualified support for the President's comprehensive program to reduce federal spending and restore stability and strength to the U. S. economy. This reaffirmation is rooted in a March 1981 letter to the President, announcing support for his economic recovery program goals, even though, as the letter made clear, the coal industry's leaders fully expected the reduction in government spending to adversely affect many ongoing research projects which promote acceptable mining and use of coal in the United States.

Last week, NCA's Board of Directors reviewed the progress of the President's economic recovery program. Also considered was the President's FY 1983 budget request for coal R&D. I can report that our Board members remain unanimous in their continuing support of the President's efforts to reduce the federal budget.

At the same time, the coal industry's leaders wish Congress to know their firm belief that budget limitations must be balanced among all energy supply options. Only a balanced energy research program can be in the nation's best interests. The economic viability and environmentally acceptable use of all our energy resources in a wise and efficient manner must be preserved through adequate research and development. Our directors also took note of statements by the Committee's distinguished Chairman and other members of the Committee, who have expressed similar views. We commend you, Mr. Chairman, and we support your commitment to seek a more balanced effort by the federal government in energy technology developments.

Achieving a Better Balance Among Energy R&D Programs

The application of the Administration's research philosophy of redirecting the federal role to long-term, high-risk activities with substantial potential payoffs is said to be the basis upon which DOE justifies a drastic reduction in fossil fuel R&D funds. The FY 1983 budget reductions relegate coal research funding to an insignificant one percent of the total energy R&D budget. In the DOE's view, the rationale for such an imbalance is explained by their saying that they phased out only those programs that have inhibited or distorted energy producer and user behavior in the marketplace.

It is difficult, if not impossible, to accept that the Department of Energy applied criteria used to define market disturbance in an equal or consistent manner to all energy fuel research programs. We believe that if they had done so properly, the FY 1983 energy research budget would have shown much better balance. Apparently, two meaningful criteria were not used by DOE in determining funding priorities. These are: (1) an analysis of our nation's energy supply options relative to the distribution and efficient use of domestic energy resources, and (2) an assessment of the progress, accomplishments and benefits of ongoing energy R&D programs to the well-being of the American consumer and taxpayer. We firmly believe that a reevaluation of DOE's budget decisions is needed, for this nation can ill afford to turn its back on coal or any viable source of energy on the basis of perceived philosophical views of what does or does not distort the marketplace.

We believe the federal government should allocate, in a responsible manner among all viable energy fuels, whatever research funds are available and that this allocation should be consistent with its role to preserve present as well as future energy options.

Coal Research Priorities

Since 1922, the coal industry has supported modest research efforts on a continuing basis at its Bituminous Coal Research, Inc. laboratory. Currently, many of the larger coal producers have their own research facilities or they sponsor projects at research institutes and universities. Much of industry's ongoing research efforts of necessity have been limited to problem solving rather than the development of new technologies for mining and using coal. There have always been, and will be in the forseeable future, limitations to the amount of research dollars that a company can afford to invest, for two reasons. First, coal markets are highly price competitive. Most small and medium size producers do not have the capabilities to conduct research. In practical terms, this establishes a limit to optional costs, like research expenses, that larger companies can incur and still compete in the marketplace. Second, coal mining or use techniques or technologies are rarely patentable, nor can they be kept proprietary and protected to prevent other producers or users from reaping their economic benefits. Under these circumstances, it is difficult for individual coal companies to commit large sums of money to expensive or high-risk multi-year research projects.

The coal industry has a long history in evolutionary development of improved mining equipment and methods. In this respect, the industry has relied on the marketplace to introduce new products and technologies. We are, therefore, supportive of the new federal research philosophy of having the private sector fund the near-term R&D. It should be recognized, however, that many ongoing federal research projects in coal mining defy simple characterizations as to their stage of development. Near-term objectives should not be confused with the readiness of the technology for commercialization.

We believe it is unrealistic for the DOE to expect the private sector to take over many unfinished coal research projects which will be terminated under the President's FY 1983 budget request. For some program areas, research progress has not yet reached milestones which would permit the private sector to adequately assess the technical risks. And, for those few projects which have reached precommercialization status and for which the marketplace suggests a viable business opportunity, the available decision-making time may be too brief. It takes time for a company to conduct the necessary technology and market assessments and secure corporate capital commitments. Under DOE's timetable, federal funding will soon be terminated and dedicated research facilities closed and technical teams disbanded.

It is fallacious for DOE to suggest that failure of the private sector to pick up projects which will be terminated by the FY 1983 budget is proof that the federal effort was misplaced in the first place. Since current market conditions affect the timing of new corporate ventures, it is unrealistic to conclude that the industry's reticence to move forward in these difficult economic times is an indictment of the usefulness of the technologies scheduled for termination. In the past, many federal coal research accomplishments have been picked up in due time by industry and developed into commercial products or uses.

The specific coal technologies which the National Coal Association has singled out in past presentations to this Committee as warranting federal support because they complement the private sector research efforts are as follows: (1) innovative extraction technologies which significantly reduce health and safety hazards to mine workers, but yet offer improved productivity; (2) coal cleaning and processing systems which reduce sulfur content, ash minerals and other undesirable trace elements in the coal prior to burning; (3) combustion systems that produce environmentally benign solid and gaseous waste products; and (4) synfuel products using conversion processes which can be environmentally controlled to protect public health and the welfare of plant personnel and local communities. The continuing need for research in these technologies, which have been the cornerstone of DOE's fossil energy program, must be reassessed more carefully than what is apparent from the justification submitted by DOE in its request for FY 1983 coal research funds.

Summary

In conclusion, an unbalanced energy research program is not in the nation's best interests. A viable federal coal research program is the key to wise and efficient use of the nation's largest source of energy fuels.

Mr. **Nelson** [acting chairman]. Thank you, Dr. Yancik.

You target innovative extraction technologies for Federal support. Are you speaking, primarily, about the long wall mining methods, or would you include advancements in room and pillar mining in your program?

Dr. **Yancik**. Mr. Nelson, I use the term "innovative" in a generic sense. There are essentially two basic methods of mining coal in this country—the room and pillar, and long wall mining techniques—the answer to your question would be yes. Innovation is needed in both systems.

We believe that there are substantial improvements that can be made in both mining techniques. This country will need both mining techniques for many, many years to come.

Mr. **Nelson**. Is the further work necessary in surface mining directed toward more cost-effective reclamation and environmental mitigation techniques?

Dr. **Yancik**. The program, as it was nearing its final stages, was concentrated in both of those areas in the Department of Energy. Towards that end, the coal industry supports that kind of work.

We believe that this is the area where there are the most immediate problems and the most immediate need to make improvements.

Mr. **Nelson**. Mr. Roach. What advanced combustion technologies do you view as the most promising?

Mr. **Roach**. Congressman, there are several different technologies which we believe hold a great deal of promise. Included among those are atmospheric fluidized bed, pressurized-fluidized bed, as well as alternative utilization processes such as coal-water mixtures.

It appears as if the AFB technology is about at the stage where industry is capable of taking it over. However, the PFB technology, which offers the potential of some lower NO_x and SO_x emissions is designated to be long-term and is in need of some additional work.

Perhaps one area that is potentially very exciting is alternative fuel utilization, like coal-water mixtures, which offer the capability of directly substituting for oil, both in utility and industrial boilers. I am sure that Mr. Anthony can expand a little more on this. The potential is something on the order of 2½ million barrels of oil per day that we are talking about and the process offers potential for immediate term.

Mr. **Nelson**. Thank you.

Let me ask you, Mr. Roach, did you hear the recommendation of the previous panel on gasification? Were you in the room?

Mr. **Roach**. I was in and out some.

Mr. **Nelson**. Whatever you heard, what is your opinion of that recommendation?

Mr. **Roach**. I did not hear so much the recommendations of the panel with respect to gasification as I did with respect to the liquid phase methanol plants. I am not sure I can really comment on that at this time.

Mr. **Nelson**. Let me ask you this. The fiscal year 1983 budget contains $1 million for the 4.8 megawatt demonstration in New York City. Is that sufficient in your opinion?

Mr. **Podolny**. I think it is marginal because it does not provide for any upsets that we may in fact observe as we try to bring the system into operation in the first instance. Then, as we try to provide the basis for guidelines for our technology and guidelines for the commercial product, I am sure it does not provide any resources for resiliency.

Mr. **Nelson**. What level do you consider to be reasonable?

Mr. **Podolny**. I think the existing level—$4.8 million—would make more sense.

Mr. **Nelson**. OK.

Mr. Walker, do you have any questions?

Mr. **Walker**. Thank you, Mr. Chairman.

Could anybody there give me an estimate of how much money we have spent in Government money in the fossil fuels area in the past 10 years?

[No response.]

I just need an estimate—$2 billion?

Mr. **Roach**. It is higher than that.

Mr. **Nelson**. Three billion dollars?

Mr. **Roach**. Perhaps.

Dr. **Yancik**. Depending on the definition of "fossil fuel" if you take in the various large sums of money which have been spent in the last several years on the demonstration plants, synthetic fuels, then the sum—and I am just now trying to recollect something that I read a couple of days ago—would probably be in the neighborhood of about $4 billion to $5 billion.

Mr. **Walker**. What if we just stuck to the areas that essentially you are testifying about here that need to be preserved under our budget. What kind of funding have we spent in those areas over the last 10 years?

Dr. **Yancik**. I think it would be something less than half of that sum.

Mr. **Walker**. So something between, probably, $2 billion and $3 billion?

Dr. **Yancik**. Approximately.

Mr. **Walker**. Can you give me some idea as to, in that 10-year period for that spending, how much new source of fuel we received out of that spending?

Dr. **Yancik**. I certainly do not want to be the only panel member responding to Congressman Walker. However, I am not certain I understand the meaning of the term "new fuel."

Mr. **Walker**. In other words, if we are spending the money for research and development, which is this committee's jurisdiction and essentially what we are talking about, what I am looking for is, over the 10-year period, how far down the road have we gotten to developing something that we would not have otherwise had and produced some actual new fuel that is replacing, say some barrels per day of foreign oil that we would otherwise be importing?

Mr. **Anthony**. I will take a crack at that.

What we have done over the past 10 years is made coal burning usable in this country. Prior to that period of the 1970's, you really could not burn coal in this country and still meet environmental regulations. We have succeeded in doing that.

Mr. WALKER. I think it was your testimony though that indicated that in so doing we have essentially utilized technology that was developed in the private sector before all the Government spending came online.

Mr. ANTHONY. Correct.

Mr. WALKER. So, in essence, that $2.5 billion has not produced or advanced any new technology and so on in that area. We may have improved what was already in place, but we have not achieved any significant breakthroughs in that area?

Mr. ANTHONY. No. What we did is we—the Federal Government's role in the 1970's was to take technology that was used to reduce pollutant emissions by 75 percent, which was very common prior to the 1970's and enhance that technology so that we could remove 90-95 percent. We would not have spent our own money doing that. We invented the basic technology to remove 75 percent. The Federal Government invested the rest of the money to bring the 75 percent removal rate up to 90 percent or 95 percent removal as called for by the Clean Air Act of 1977.

Mr. WALKER. I wonder, if the Federal Government had stuck to its guns on the Clean Air Act, whether somebody would have decided that they needed to do something in terms of technological breakthrough just by being pushed by the regulatory standards that were coming in?

Now, they may well have switched to oil which would have given us additional problems in terms of importation, but on the other hand, if the technology was already basically in place I think it can also be assumed that somebody would have picked up and moved that forward.

What I am trying to get to here, obviously, is that I am wondering, we have at least 10 years of major investment, to the tune of some billions of dollars and what I would like to know is, over that 10-year period, what has the Government produced for its money? What kind of new fuel sources do we have on line? What kind of environmental breakthroughs have been made? Just what, in your opinion, has the Government spending accomplished at this point which justifies us continuing it at high levels of spending for, say, the next 10 years?

Dr. GUMNICK. May I say a word?

Mr. WALKER. Yes.

Dr. GUMNICK. I do not want to try to speak to all of the fossil fuels, but to the case of enhanced oil recovery with which we are quite familiar. It was back in 1974 that we were first asked by a combination of the National Science Foundation and what was then the Bureau of Mines to assist in putting together a research plan for the Federal Government to implement an enhanced oil recovery research program.

Mr. WALKER. So it has been 8 years?

Dr. GUMNICK. No, that was when we were asked to begin to put together a plan. The plan was completed in 1977 and elements of that plan are still lacking in execution.

What has happened is that there have been a number of field tests that have been funded. At the time, there was off-the-shelf technology that was implemented in 22 programs that were encouraged by Federal action. These went into effect during that period of time. It takes over 6 years for an enhanced oil recovery program to be evaluated from the point of view of the technical excellence of the program and the economic viability of the program.

Because it will take 6 years, and I am talking about initiatives in the period 1978, 1979, 1980, for many tests we are not even there yet.

Another program was initiated which is the incentives program that led to 400 new projects starting up last year. The amount of money that is being put in the present budget is not even sufficient to monitor what is going on in those programs which will not have payoffs for another 3 to 6 years.

Mr. **WALKER**. What I am getting at here now is that the Federal Government first got into this in 1974 with the study. Beginning in 1977——

Dr. **GUMNICK**. Something began to happen, money began to be invested.

Mr. **WALKER**. OK, so money began to be invested. Given the fact that I understand that there are long lead times involved and so on——

Dr. **GUMNICK**. We are talking about long-range, high-risk research.

Mr. **WALKER**. OK, here we are, we are at 1982 and we have not yet, as I understand your testimony, produced one barrel of enhanced oil recovery as a result of our commitment.

Dr. **GUMNICK**. No; we have produced barrels of enhanced oil recovery but the barrels are limited. Right now there are about 400,000 barrels per day being produced by improved water flood, thermal and in-situ combustion processes. It represents about one-half of the oil that is being produced in the State of California and these are relatively well tested processes.

We know that enhanced oil recovery can work. The question is how do you make it work effectively and economically and that is the issue that we are speaking to. I would want to reduce the risk.

Mr. **WALKER**. I think that there is a need and I happen to believe very strongly in the fact that we ought to be balanced in what we do in our energy research and development, but I submit that part of the problem in defending this kind of action is that when we look over the past and we do not see the kinds of developments that are bringing all these things to bear we begin to wonder whether or not the Government is a help or a hindrance in some of these things. I do not think there is any guarantee that if we spend another 5 or 6 years doing the same thing that we will not come up with the same results. We will be down at the end of that period of time, we will have spent the money, and we will still be saying, we hope that in 5 or 6 years from now we are going to have something to show you.

I have been on this committee for 6 years and I have been hearing it for 6 years.

Dr. **GUMNICK**. If we apply the same principles that you are talking about to the national defense, we would get out of the defense business because we have long lead payoffs. Where is the B-1 bomber? Where is the MX missile?

Now let me back up to a fundamental point which I tried to make in my speech but maybe was lost.

There appears to me to be a very reasonable and rational role in the Federal Establishment to put a fly wheel on the vacillations of the marketplace. We all talk like we are working in or toward a free market in the face of one of the greatest cartels that ever existed OPEC. In self defense we should put a fly wheel on the machinery of research.

Three or four years ago we were beating the bushes to get university people engaged in enhanced oil recovery—3 or 4 years ago; that is how long it takes for a thing like this to get going. Now they are beginning to do some really good work and the establishment says, sorry, Charlie, we do not need you any more. In about 3 or 4 years are we going to come back and say, on second thought, we need you again. In good research you just cannot stop it and start it at whim. We are not talking about enormous amounts of money compared to some standards. At a time when we are avidly focusing our attention on national defense, we are doing the equivalent of unilateral disarmament in the energy battle. If we continue it is going to get us into a desperate situation.

Mr. **Walker**. I think that is a very legitimate concern. I guess that part of our problem, in trying to assess where we go on this is to figure out where we begin to set some priorities as well and where we do have some chance of producing some real results for the investment we are making, particularly when funds are becoming more limited. That is a key question here and the reason why I think we, at this committee level, need to begin to raise the question of, what has been done with our money that we have invested in all these things in the past? Where have we produced some real results? Where do we have the hope of producing some real results in the years just ahead? That is the reason for the question and I would really like to know what results, not only have we produced thus far, but what results can we expect in a very, very near term, given the fact that we have limited resources to invest, as you say, in what is almost a national defense kind of a climate. I think we have to begin to show some results for all of this Government involvement.

Dr. **Gumnick**. I want to make one other point that might answer your question.

I referred to the amounts of money that are being tapped out of the windfall profit tax. The numbers that I read in the budget, under "excise taxes" were numbers like $24 billion and $21 billion. Now, as I recollect, when we were all talking about a windfall profit tax we said that one of the reasons we really need it is so that the Government can see to it that we never get into an energy bind again. Then the windfall profit tax was dumped in the general budget, not energy research and development. If you pull that $25 billion out of the revenues, you would have a $125 billion deficit. That money was tapped out of energy capital. Moreover, let us please be careful about making arbitrary distinctions between energy companies, oil companies, or whatever. Energy companies are energy companies. The oil companies are in all fields of energy, including nuclear, so that when we pull that so called "oil money" out of the industry we are pulling capital out of the total energy environment.

Mr. **Walker**. I would say to you that it is rather controversial that we put some of that money back in last year's tax bill. You know the windfall profit tax was substantially modified in terms of revenues in last year's tax bill and it is a subject of a great deal of controversy on the House floor right now.

Dr. **Gumnick**. Right. So you are forcing decisionmaking in a very unstable situation and corporations will not take risks in unstable situations.

Mr. **Walker**. Granted. The question is, though, when we are faced with a $100 billion deficit whether or not the Federal Government can continue to take risks without setting some very, very well-defined priorities.

I thank you, Mr. Chairman.

Dr. **Yancik**. Mr. Chairman, may I make one further brief comment regarding the basic response to Mr. Walker's question?

Mr. **Nelson**. Certainly.

Dr. **Yancik**. He asked, "What are the accomplishments"?

I think that the point is that research is very long term but if you look at many of the regulatory requirements that have been imposed on the energy-producing industry, namely those that produce energy—electricity—by burning coal, they were faced with enormous difficulties in meeting environmental requirements.

These regulations that were imposed were essentially technology-forcing regulations. The technology did not exist although we did have some of the basic knowledge and know-how to do some things to reduce the amount of particulates, to reduce the oxides of nitrogen and sulfur. There were no commercial techniques—you could not go and order something and get a guarantee that it would be able to do this. Then enormous amounts of money were spent by the Federal Government in these areas. They have led to systems that are available commercially but they still have problems and there are still concerns that have not been resolved fully but this country is moving along. I think that if you just look in terms of the increased use of coal, that gives you one answer. We are now producing 52 percent of our electricity in this country from coal and we are doing it with less total emissions of sulfur oxides and particulates than we have ever had before, so I think that is part of the answer.

Mr. **Walker**. If I could, Mr. Chairman?

Mr. **Nelson**. Surely.

Mr. **Walker**. I thank you for that answer, but again I come back to the testimony that Mr. Anthony gave us that the technology that was used was essentially technology that was in place—it was obviously upgraded and we found better ways to use it—but the question for me is whether or not this committee's spending for research and development is involved in that when, in fact, we are basically upgrading old technology?

Consider what is mentioned in your testimony and Mr. Anthony's testimony about the new generation of equipment that would be far better than that. Is not that what this committee should be bringing online and should it not have brought it online within the last 10 years of the spending that we have done?

Mr. **Anthony**. Well, the tasks listed in the coal utilization budget now offer new technology. Mr. Roach mentioned several and I mentioned several. We are not talking about investing money now in old technology. We are talking about developing chemically clean coal, coal-water mixtures which have tremendous potential, I believe. Imagine a clean coal, cleaned of ash, cleaned of sulfur and mixed with water, burning it instead of oil. That is the potential; that is the promise. The savings involved in that kind of technology would obsolete our own equipment—the equipment that was designed for the past 70 years would be obsolete in the 20 years because it would be more cost effective. That is what you are asked to support.

Mr. **Walker**. Also, though, I would say that when you take a look at the administration's position on this, I think what the administration might be saying, at least in part, and I do not go along with everything that they have put, in terms of nickels and dimes, what they have put in the budget. However, I think they may be saying in part, philosophically, they think that the money would have been more wisely spent 10 years ago doing precisely this kind of work 10 years ago rather than using R. & D. money to give retrofits to essentially old ideas.

I am not so certain that the philosophy behind the administration is not correct in determination of where the priorities should lie in terms of the budgeting process in order to assure that the direction we go is toward a new technology and not just new gimmicks to hang on to old technology.

Mr. **Anthony**. I would have been very happy if we had stayed with the Clean Air Act of 1970; I think we would have made much more progress across this Nation. As a matter of fact, what I would like to see done is the Clean Air Act of 1981 or 1982—I should say 1982 or 1983—freezing macroparticulate standards and sulfur dioxide standards at some appropriate level. Then, I think, the rest of this industry of ours would have confidence that there was a continuity ahead of us in regulations that would make the market attractive for investment.

Mr. **Walker**. In new generation equipment.

Thank you, Mr. Chairman.

Mr. **Nelson**. Yes, indeed, Mr. Walker.

Gentlemen, we want to thank you very much for participating in this panel and in bringing your expertise to us.

[Whereupon the meeting was adjourned at 12:02 p.m.]

[Additional statements for the record follow:]

ADDITIONAL STATEMENTS SUBMITTED FOR THE RECORD

TRW

T006-82.021

9 April 1982

The Honorable Don Fuqua
U.S. House of Representative
Washington, D.C. 20515

Dear Chairman Fuqua:

TRW has long been committed to enhancing existing energy sources and to developing new ones. We have participated in the Nation's Fossil Energy Research and Development Program for a number of years and so we are particularly interested in your subcommittee's hearings on Energy Developments and Applications of 22 and 23 March, 1982. Although TRW did not testify at those hearings, we would like to present our position on the Fossil Energy R&D Program for your consideration and possible inclusion in addendum to the hearing report of these hearings.

For your information, every TRW program (8) mentioned in the opening paragraph of the attached statement will be terminated if the FY 1983 DOE Fossil Energy budget remains at the requested ($107M) level.

Sincerely yours,

John R. Sellars

John R. Sellars
Vice President and General Manager

JRS:bl

ENERGY DEVELOPMENT GROUP OF TRW INC • ONE SPACE PARK, REDONDO BEACH, CALIFORNIA 90278 • (213) 535-4321

STATEMENT OF
JOHN R. SELLARS
MARCH, 1982

TRW has long been committed to enhancing existing energy resources and to developing new ones. In the Fossil Energy area we are developing advanced utility and industrial coal combustors and gasifiers. Our Gravimelt process is removing 90% of the sulfur and ash from eastern and mid-western coals. Our BEACON venture with SOHIO promises to convert low-BTU gases to hydrogen or cogenerate methane and electric power. We are conducting magnetohydrodynamic combustor R&D and are progressing toward operation of an advanced MHD power train. We are also active in oil shale development, coal-bed methane recovery and in-situ coal gasification, all in support of this nations effort to better use our domestic resources. To this end, we have invested more than 15 million dollars in Fossil Energy technologies over the last six years, including the construction and operation of our own Fossil Energy Test Site near San Juan Capistrano, California.

I would now like to make a few remarks on the subject of our Nation's fossil energy research and development program. The budget I have seen for FY 1983 is nearly a factor of ten lower than the budget of only a year ago, FY 1981. While some tightening up may have been in order, in my opinion, such a reduction is too severe for the national good.

Let me review a few reasons why I believe in a strong national Fossil Energy R&D program: First, there is the incalculable worth of our resources (oil, tar sands, shale, and particularly coal). Their value can be realized only if we convert these resources to useful forms in an environmentally acceptable way. Clearly we have the energy in the ground for complete independence from foreign sources; indeed, we can become major energy exporters to the rest of the world. If our environment is destroyed or our national treasure of energy is wasted by inefficient recovery or usage, we will leave a poor legacy to our children.

Environmentally, cleaning up coal or converting it to other forms will surely be cheaper, for the economy, than stack gas scrubbers or acid rain on our land.

Second, the national security has an economic as well as a military side, as the events of the last decade have shown. At this moment, there seems to be a perception that the energy crisis is over and that the free market will take care of any future problems. Yet, we hear that future plans for synfuels and shale oil are being abandoned or delayed by private companies because of the present uncertainty in the price of crude oil. But with all of the drilling last year and the year before, no reports of "super-large" new oil fields have been made. In fact, U.S. new oil discoveries have leveled out. Corporations give very heavy weight to the short term effect on their profits and must do so almost by their nature; the federal government must therefore supply the necessary bridging continuity.

Third, the FY 1983 budget that I have seen states that the government is no longer going to assist in merely bringing energy technology to the market "earlier". If one feels that there is still an energy crisis, then bringing energy technology to the market "earlier" is precisely the point. In many cases some of the technology abandoned under such a policy will never reach the market in the forseeable future. Before it will pursue a new technology, industry must see a convincing demonstration of that technology and thus be assured of a reasonably return on investment. This is economics as it is practiced in the market place and it is certainly consistent with the free market philosophy. The U.S. government's role is to expedite the improved use of coal, making it a more valuable resource for U.S. industrial investment.

Finally, there is the crucial point of maintaining a consistent effort. A case in point is one of the most startling transformations of modern American society which resulted from a long-term, wide-spread research program sponsored by both our State and Federal governments. I refer to the change which took place in American agriculture in developing new strains of plants and animals, and in the technique of farming in general. This research program was carried out in colleges, universities, industry and government laboratories all over the country for many decades, and this research made the American farmer the most productive in the world. This could have not been done by on-again, off-again, start-stop programs. Nor can energy research and development be carried out without continuity and consistency. Many producers, distributors and users of energy are too small to carry out the needed development, just as individual farmers were. Even some of the larger entities, such as utilities, do not have the necessary technical staff to carry on these developments; even if they did, they are too strapped financially to respond alone.

In conclusion, I would like to point out that even during a global recession, Japan, the People's Republic of China, the USSR, Poland, Italy and Germany are continuing with government-funded fossil energy R&D. They will therefore reap the economic benefits of the technology and enhance their worldwide technical prestige. Without a strong, adequately funded, U.S. Fossil Energy Program, foreign nations will continue to usurp our technological world leadership, and as with the automobile, will surely in time erode the American Fossil Energy Equipment market, both at home and abroad.

I thank you, Mr. Chairman, and the committee for your consideration.

BIOGRAPHY OF

DR. JOHN R. SELLARS

Dr. John R. Sellars is the Vice President and General Manager of the Energy Technology Division of TRW. This Division has responsibility for work in the chemical, nuclear, and combustion areas for the Energy Development Group, Redondo Beach, California.

Dr. Sellars joined TRW (then Ramo-Wooldridge) in 1955 and participated in re-entry vehicles work in the ballistic missile program. He was appointed Manager of the Aerodynamics Department in 1958. This Department was responsible for re-entry and launch aerodynamics, decoy and fusing problems, and various special fluid mechanics problems in connection with ballistic missile and space vehicles work. Subsequently, he has handled various Operations in TRW leading up to his last position as Manager of the Research and Technology Operations, a position he has held since 1971. This position included the Lead Executive responsibility for all Energy activities in the Applied Technology Division.

Before joining TRW, Dr. Sellars was assistant professor of Aeronautical Engineering at the University of Michigan. While teaching, he engaged in analytic investigations on heat transfer problems, stability problems of flows and problems in the determination of laminar flame speeds.

Dr. Sellars received his Ph.D. degree in Aeronautical Engineering from the University of Michigan in 1952. His thesis dealt with "Some Special Problems in the Stabiligy of Laminar Flows". While a graduate student, he participated in Project Wizard and in research programs, including wind tunnel work on the pressure distributions over ramjet models buring hydrogen. He received his M.S. degree in Physics from the University of Michigan and his B.S. in Electrical Engineering from New Mexico State University.

Dr. Sellars is the author of numerous technical papers and has one patent for a Re-entry Vehicle design in 1961. He received the Sesquicentennial Award in 1967 from the University of Michigan College of Engineering. John and his wife Ethel have three grown children and reside in Redondo Beach, California.

BARRY M. GOLDWATER, Jr.
20th District of California

COMMITTEE ON PUBLIC WORKS
AND TRANSPORTATION
COMMITTEE ON SCIENCE AND
TECHNOLOGY

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April 26, 1982

The Honorable Don Fuqua, Chairman
Subcommittee on Energy Development and
Applications
B374 Rayburn House Office Building
Washington, D.C. 20515

Dear Don:

I would like to request that the enclosed statement on the Magnetohydrodynamic (MHD) power generation program be included in the MHD hearing record. The statement is submitted on behalf of Mr. S. T. Demetriades who is President of STD Research Corporation.

Please let me know if this is possible. Thank you for your attention to this matter.

Sincerely,

Barry

BARRY M. GOLDWATER, JR.
Member of Congress

Enclosure
BMG/sr

STATEMENT OF
S. T. DEMETRIADES
PRESIDENT AND TECHNICAL DIRECTOR,
STD RESEARCH CORPORATION, ARCADIA, CA 91006
TO
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS, THE COMMITTEE ON SCIENCE AND TECHNOLOGY, U.S. HOUSE OF REPRESENTATIVES
ON THE SUBJECT OF MAGNETOHYDRODYNAMIC (MHD) POWER GENERATION

There is not much doubt that the nation's ability to compete on a world scale requires technical superiority and technical excellence. Technical superiority and excellence may come about either by a lucky, quick breakthrough - a brainstorm - or by patient, deliberate, long-term dedication and effort or by any combination of these two extremes. I suspect that in the mature technologies a quantum-jump of success is more a case of long-term effort than sudden inspiration or luck. However, in the sophisticated technologies even a brainstorm requires long-term immersion in a subject and a strong understanding of its fundamentals. Energy technology is a mature technology. Magnetohydrodynamic (MHD) power generation is a sophisticated energy technology.

Progress in MHD has required patience and time. There is no doubt that thanks to government funding progress has been made in MHD.

The private sector, with its inability to sustain a long-term effort before showing profit, cannot support MHD at an adequate level. In the same manner, Detroit failed to support a rational long-term development effort in fuel-efficient automobiles. As a result, Detroit lost its leadership in the production of automobiles.

The Congress has been a strong supporter of fossil fuel research and development and particularly of MHD. A quick way to kill any chances this country has to maintain a competitive position in the fossil fuel energy technologies and especially MHD would be for the Congress to agree now with the administration's FY 1983 budget proposal for the DOE's fossil fuel program.

Think about the role of government in research and development. How did the airlines and aircraft industry make it? How did the computer industry make it?

Without government research and development support the U.S. would not be the world leader in aeronautics and the production of commercial airliners. Without the government's role through the space and defense programs in developing new computer technology we would still be doing our computing with the slide rule.

By all means reduce government spending but don't kill the kernel, the seed, of the good and necessary programs along with the wasteful ones. Above all don't kill a program for dogmatic reasons. Think of the consequences. It took a long time to build our technical competence in magnethydrodynamics.

Shutting down the MHD program may save a few dollars today but the future cost to the nation will be great. It took twenty or more years to learn how to do MHD. If the program is killed now, the trained people and the art will be lost, to all intents and purposes, because this sophisticated technology has not been around long enough to leave a good imprint. Not only will money be wasted, but trained minds as well - an awful thing to waste. It takes a long time to train an engineer or scientist to be competent in MHD.

The nation's military and economic strength and quality of life are interconnected to its energy use and utilization. Our nation's present economic crisis, the high inflation rate and associated prohibitive interest rates must be controlled to reduce their direct and indirect cost to our citizens - both in their spending to meet their personal needs and as taxpayers to pay for government spending. The long-term solution for limiting the large outflow of material assets to pay for foreign oil and increasing penetration of domestic markets by foreign suppliers requires a continuing national commitment to better utilization of domestic energy resources and to the development of the country's technological capabilities. To curtail this commitment and demolish this capability, which took twenty years to build up, for minor immediate budget savings will only magnify in the long term the economic crises of the nation.

Environmentally desirable and efficient utilization of coal, the nation's most plentiful energy resource, is a paramount and vital long-term research and development objective. There is no doubt that direct coal-fired magnetohydrodynamic (MHD) power plants offer the best long-term solution. MHD's high efficiency will minimize both the required expansion of the nation's mines and railroads to supply the coal and substantially reduce the waste heat and pollutants produced per kilowatt-hour generated. MHD's projected low electric power costs should repay the consumers (the taxpayers) many times over for the government's investment to demonstrate proof of the MHD concept. The government's participation is essential to reduce development costs and time to acceptable values for subsequent industrial development.

Under the Department of Energy program, outstanding progress has been and is being accomplished on all aspects of MHD technology: coal combustors, generator performance and life, superconducting magnets, steam boiler design and control of stack emissions, plant configuration design, computer modeling and simulation to understand and scale the MHD power plant and facilities for ultimate proof-of-concept tests. MHD technology is also important to a broad range of other fossil energy applications. But more significant is it's direct application to potentially important new national security needs. Light-weight, high-power MHD systems could attractively satisfy many space and terrestrial missions being considered by the Department of Defense.

Termination of the Department of Energy MHD program would cripple this nation's capability in MHD technology and send us back a decade or more compared to the continuing vigorous USSR MHD development program. The uncertainties and disruptions in MHD funding in the past two years also have been harsh because of the associated waste in the limited funds available to support MHD. The nation needs a continuing, firm and reliable, long-term commitment to MHD development. The MHD program, at the very leading edge of technology, may offer the greatest opportunities for serendipity and synergy. The spin-offs from MHD may be exactly what this nation needs to revitalize its industry.

There is language in the present OMB Budget submission for FY 83 to initiate termination action with respect to the National Magnetohydrodynamics (MHD) Program (part of the Fossil Energy Program) on 1 April 1982. (See Congressional Budget Request, FY 1983, Volume 5, DOE/MA-0057, February 1982, pages 65 and 66).

It is not appropriate to initiate this action without the consent of Congress and the OMB is not giving Congress sufficient time to act on this matter between now and 1 April.

Terminating the MHD effort in the face of the known Soviet advances in this most important field of power generation makes a mockery of the President's efforts to dissuade the Europeans from using Soviet natural gas. If the U. S. MHD program is closed down, the Soviets will have a monopoly on the technology to burn natural gas and generate electricity twice as efficiently as the power-generating equipment manufacturers in the U.S. Moreover, they will be able to do so with the direct burning of coal, as well as natural gas, in an environmentally acceptable way (no acid rain, etc.).

Thus we are very rapidly reaching the point where the Soviets will not only be able to penetrate Europe's natural gas market but also the far more dangerous point where they will be able to monopolize the technology for efficient power generation from fossil fuels!

Therefore, for reasons that range from National Security and the nation's foreign policy to conservation, from energy self-sufficiency to a cleaner environment, and from modernization of traditional industries to earthquake prediction, I urge you to give strong support to the National Program for Magnetohydrodynamics.

For these reasons, we request that Congress continue to fund the MHD program in FY 1983 and direct the administration to carry out this program.

AVCO EVERETT RESEARCH LABORATORY, INC.

A SUBSIDIARY OF AVCO CORPORATION

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EVERETT, MASSACHUSETTS 02149
TELEPHONE 617 389-3000

VINCENT J. COATES, JR.
DIRECTOR OF SPECIAL PROJECTS

March 19, 1982

The Honorable Don Fuqua
Chairman, Subcommittee on Energy Development
and Applications of the
Committee on Science and Technology
U.S. House of Representatives
Washington, DC 20515

Dear Mr. Chairman:

We are pleased to submit the attached statement which describes the status of the MHD Technology Development Program.

Sincerely,

Vincent J. Coates, Jr.

VJC:cg
Attach.

Summary of Statement
Submitted by

Dr. Robert Kessler
Vice President
Energy Technology Office

Avco Everett Research Laboratory, Inc.
2385 Revere Beach Parkway
Everett, Massachusetts 02149

for

Subcommittee on Energy Development and Applications
of the
Committee on Science and Technology
United States House of Representatives

23 March 1982

Very significant progress was made during the past year in the development of MHD power trains (combustor and channel or generator). A 1000-hour channel test was conducted at Avco Everett Research Laboratory, Inc. (AERL) under coal fired commercial power plant conditions. The test demonstrated that electrode lifetimes of up to 8000 hours are possible. (Electrodes are the most critical part of an MHD generator). This test result meets a utility industry requirement for early commercial power plants.

Experiments conducted at the Arnold Engineering Development Center achieved performance levels of 10.5% enthalpy (electric energy) extraction which confirmed the theoretical predictions which are the basis for projections of commercial MHD power plant performance. The test, conducted on a generator about half the size needed for an early commercial MHD plant, provided good confirmation that MHD components can be scaled to commercial size.

An MHD coal combustor developed at TRW will begin actual coal fired operations with an MHD generator at 20 MW levels at AERL later this spring. These tests will provide engineering and performance data for the operational demonstration of a 50 MW power train scheduled for integrated tests at the Component Development Integration Facility (CDIF) in 1983.

The University of Tennessee operated a 28 MW MHD generator, resulting in NO_x and SO_x levels well below EPA's new source performance standards. Argonne National Laboratory built and successfully tested a 200 ton, 6.2 Tesla superconducting magnet.

It can therefore be concluded that if properly funded, the technology development program now being conducted by DOE's MHD office will make it possible to begin the commercialization phase of MHD development within the next four to five years.

Statement Submitted by

Dr. Robert Kessler
Vice President
Energy Technology Office

Avco Everett Research Laboratory, Inc.
2385 Revere Beach Parkway
Everett, Massachusetts 02149

for

Subcommittee on Energy Development and Applications

of the

Committee on Science and Technology
United States House of Representatives

23 March 1982

MHD is a direct energy conversion process which offers distinct and important advantages for the production of electricity from coal. In the MHD system, coal is burned to produce combustion products at about 4500°F, which are seeded with a potassium salt. Under these conditions, it is a good conductor of electricity and in accordance with Faraday's principle, generates electricity as it passes at high speed through a magnetic field. The principal components of an MHD flow train are a combustor, an MHD channel, or generator, and a magnet. While not directly involved in the electric generating process, environmental control of an MHD power plant is also important.

The inherent mechanical simplicity of the MHD generator, with no moving parts, makes it possible to burn coal directly and to handle very high temperature combustion gases containing ash impurities which would quickly destroy a turbine. Because of its high temperature capability, a much more efficient use of the fuel is possible. Also, since the coal is burned directly, there is no loss associated with prior coal gasification or liquefaction. Numerous studies show that mature MHD power plants will produce 50% more electricity from the same amount of fuel as compared to the most efficient conventional steam power plants in use today. It is important to note that the more efficient use of the fuel in MHD power plants will be coupled with significant reductions in environmental intrusion. Experimental and analytical work has verified that MHD power plants can be designed with pollutant emission levels far below existing and future environmental standards and regulations.

I would like to stress that there are no fundamental scientific obstacles to the development and commercialization of MHD power generation. MHD requires an aggressive engineering program leading to a demonstration of commercial power plant operation.

In the following sections, I will discuss the status of each of the principal components of MHD power generation and more briefly the economic and environmental advantages.

Channel

The MHD channel is the heart of the MHD power generation system, since it is the component which produces the electric power and its requirements determine the specifications for other components and subsystems of the MHD power plant.

The major issues in channel development are whether adequate efficiency and enthalpy (energy) extraction can be achieved simultaneously and for sufficient duration to satisfy requirements for commercial operation. In 1981, significant steps were taken toward addressing these technical issues.

With respect to long duration channel operation, the most significant event in 1981 was the successful completion at Avco Everett Research Laboratory of a 1000-hour generator test (Figure 1), conducted under the same chemical, electrical and thermal stress conditions which would exist in a coal-fired commercial power plant. The test demonstrated that electrode lifetimes of up to 8000 hours are possible (Figure 2). This result meets a utility industry requirement for early commercial MHD power plants. Electrodes are the elements of the channel which draw off the electricity and are the most critical part of the MHD generator. The 1000-hour test also represented a major advance in Avco Everett's long duration MHD generator experience, following successful completion of tests of 100 hours in 1976 and 500 hours in 1978 (Figure 3).

With respect to enthalpy extraction, the Arnold Engineering and Development Center (AEDC) has achieved performance levels of 10.5% enthalpy extraction with a magnetic field of 3.2 Tesla in preliminary runs of its 290 MW generator.

This result is significant for two reasons. First, it is the latest in a series of experiments, which confirms theoretical predictions for commercial MHD power plant performance; and second, the generator was of large enough size (about half the size of a generator for an early commercial plant) to provide good confirmation of MHD generator scaling laws. Verification of scaling laws has been an important objective of MHD testing, as the relatively small scale generators operated to date cannot achieve the performance projected for larger channel of commercial size. However, as shown in Figure 4, the experimental results

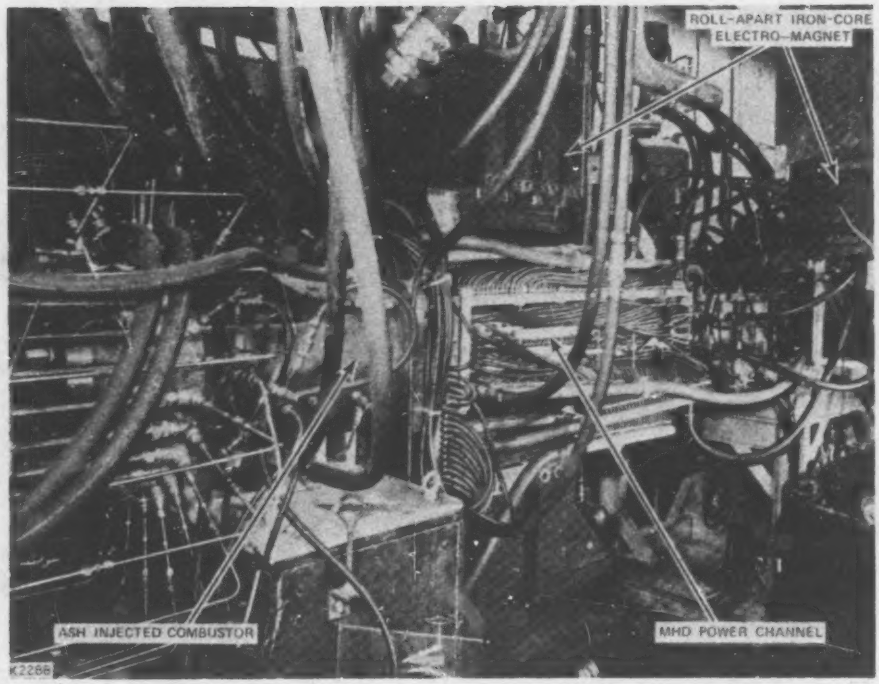
ROLL-APART IRON-CORE
ELECTRO-MAGNET
ASH INJECTED COMBUSTOR
MHD POWER CHANNEL
K2288

Figure 1

The 1000-hour test was completed on the Mark VII MHD power train located at Avco Everett, which operates the most extensive industrial MHD test facility in the country.

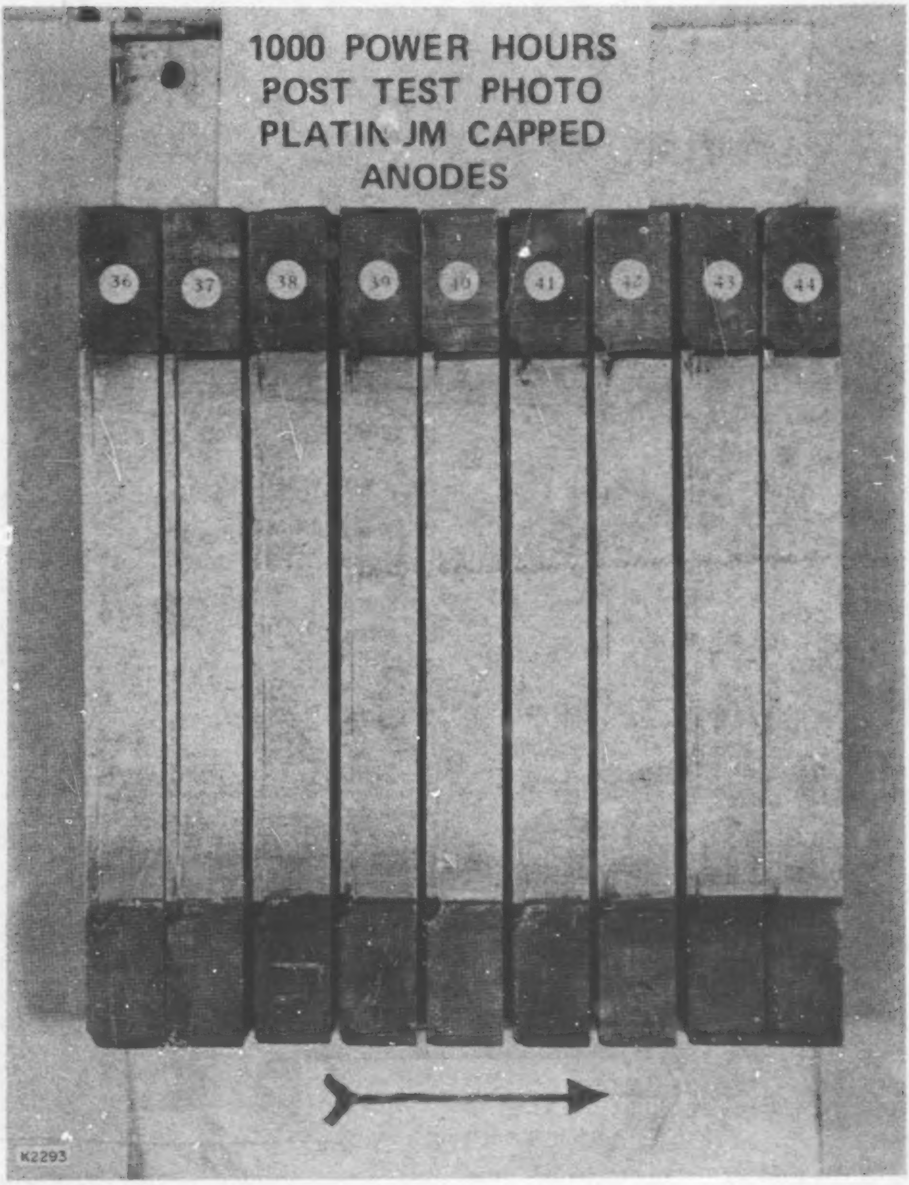
1000 POWER HOURS
POST TEST PHOTO
PLATINUM CAPPED
ANODES
36
37
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Figure 2

Section of channel wall, showing several electrodes which completed the 1000-hour test at Avco Everett.

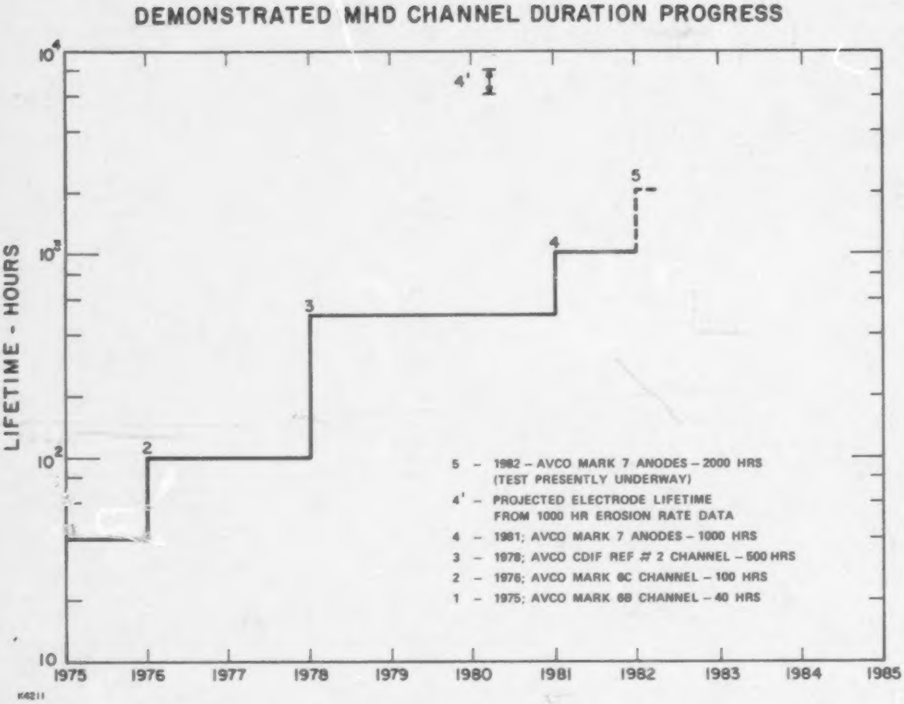
DEMONSTRATED MHD CHANNEL DURATION PROGRESS
LIFETIME - HOURS
10
10^2
10^3
10^4
1975
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1
2
3
4
4'
5
5 – 1982 – AVCO MARK 7 ANODES – 2000 HRS (TEST PRESENTLY UNDERWAY)
4' – PROJECTED ELECTRODE LIFETIME FROM 1000 HR EROSION RATE DATA
4 – 1981; AVCO MARK 7 ANODES – 1000 HRS
3 – 1978; AVCO CDIF REF # 2 CHANNEL – 500 HRS
2 – 1976; AVCO MARK 6C CHANNEL – 100 HRS
1 – 1975; AVCO MARK 6B CHANNEL – 40 HRS
K4211

Figure 3

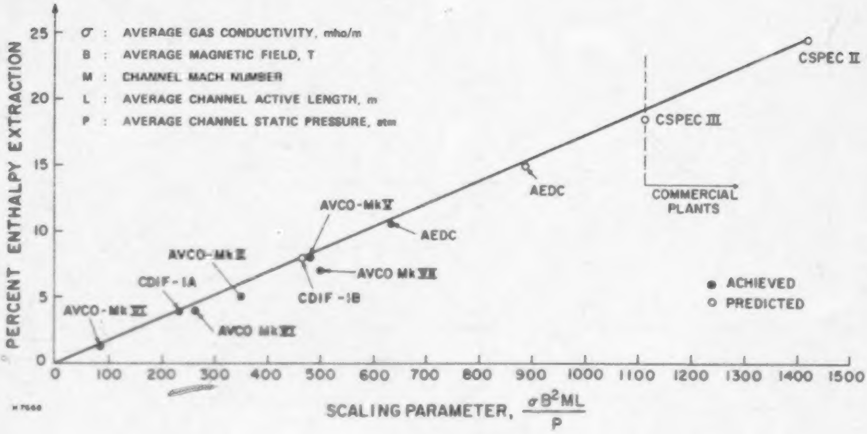
σ : AVERAGE GAS CONDUCTIVITY, mho/m
B : AVERAGE MAGNETIC FIELD, T
M : CHANNEL MACH NUMBER
L : AVERAGE CHANNEL ACTIVE LENGTH, m
P : AVERAGE CHANNEL STATIC PRESSURE, atm
PERCENT ENTHALPY EXTRACTION
0
5
10
15
20
25
0
100
200
300
400
500
600
700
800
900
1000
1100
1200
1300
1400
1500
SCALING PARAMETER, $\frac{\sigma B^2 ML}{P}$
CSPEC II
CSPEC III
COMMERCIAL PLANTS
AEDC
AVCO-Mk V
AEDC
AVCO-Mk II
AVCO Mk VII
CDIF-1A
CDIF-1B
AVCO-Mk VI
AVCO Mk VI
● ACHIEVED
○ PREDICTED

Figure 4

lie on a line of expected performance which is drawn on the basis of analytical calculations and which leads to performance required for commercial plants.

Coal Combustor

The MHD coal combustor must produce homogeneous, seeded combustion gases at high thermal efficiency as well as withstand the electrical potential of the MHD channel. Two design concepts for the MHD coal combustor have been designed in parallel. The first has been developed by the University of Tennessee Space Institute and utilizes a single stage combustor in which slag is not removed from the combustor. The combustor delivers all the coal mineral matter through the MHD power train to a downstream recovery system in which ash and seed are recovered and separated. The second concept, in which slag is removed from the combustor, has been developed at laboratory scale by three contractors, TRW, Avco Everett and Rocketdyne.

Later this spring, TRW's two stage combustor will be interfaced with an Avco channel at the Avco Everett Research Laboratory to begin actual coal-fired operation of an MHD flow train at the 20 MW level. These tests will provide engineering and performance data for the operational demonstration of a 50 MW power train scheduled for integration tests at the Component Development and Integration Facility (CDIF) in 1983.

Superconducting Magnets

Superconducting magnets for early commercial MHD plants will have a stored energy of between 1000 to 2000 megajoules and will need to provide magnetic fields of 4.5 to 6 Tesla. The technology for such devices is well developed. For example, a fusion experiment superconducting magnet at Livermore Laboratory, which has a more complicated geometry than an MHD magnet, has successfully been operated at a peak field of 7.8 Tesla and has a stored energy of 500 megajoules. Another superconducting magnet presently under construction at Livermore will have a stored energy of 1600 megajoules.

The engineering of superconducting magnets for MHD application has also accelerated in the past year. Argonne National Laboratory in Illinois built and successfully tested a 200 ton s.c. magnet intended for use at the Coal Fired Flow Facility in Tennessee. The magnet generates a peak field of 6.2 Tesla, which exceeds the magnetic field requirement for commercial-scale MHD plants. Also, a 6 Tesla s.c. magnet is being designed and built by the General Electric Company in New York for integrated testing of the power train at the CDIF.

The first successful superconducting magnet for MHD application was built by Avco Everett in 1966. Since then, a s.c. magnet was operated with an MHD channel at the Electrotechnical Laboratory in Tokyo, Japan, and another at the U-25B facility in Moscow. This latter magnet was built by Argonne National Laboratory as part of a U.S.-U.S.S.R. cooperative program, and prdouced a peak central field of 5 Tesla.

Heat Recovery and Seed Recovery

The heat contained in the MHD generator exhaust gas is recovered in a bottoming plant for generation of steam for additional power production and for preheating of the combustion air or oxidizer used for burning of the fuel. In addition to recovery of heat, seed is also recovered in the bottoming plant for economic operation.

The bottoming plant heat and seed recovery system serves also the important function of reducing the content of nitrogen oxides in the gas. NO_x emission control is accomplished by use of two-stage combustion with partial combustion in the MHD combustor and completion of combustion in the bottoming plant steam generator. Experimental and analytical work has shown that the NO_x emission level in the gas can be controlled to very low levels which are only a small fraction of current EPA standards.

Seed recovery is combined with removal of sulfur from the gas. This occurs because the potassium seed will react with the sulfur contained in the gas to form potassium sulfate, which condenses on the boiler walls and is removed. Experimental work has demonstrated that essentially complete sulfur removal from coal combustion gases can be achieved in MHD power plants. This will permit MHD power plants to burn coal of any sulfur content with emission control of sulfur oxides produced in combustion of the coal. Very efficient removal of condensed seed and remaining fly ash in the gas has also been experimentally demonstrated. This assures that efficient recovery of seed can be attained and that emission of particulates is controlled and reduced to levels below EPA standards.

Component Demonstration and Integration

Two government owned, contractor operated test facilities have been commissioned and are presently conducting integration tests of the MHD components supplied by industrial contractors from across the country.

The Component Development and Integration Facility (CDIF) located in Butte, Montana is a complete 50 MW MHD power train test facility. Shakedown tests of an MHD generator supplied by Avco Everett have met design power performance using an ash-injected combustor also supplied by Avco. An EPRI sponsored DC/AC Inverter subsystem is being installed, on schedule, for operational tests in which power will be delivered from the MHD channel to the Montana power grid, through the inverter, in the spring and summer of 1982. TRW in California is completing final design of a 50 MW coal combustor and will make delivery before the end of 1982. Operational data from the integration and demonstration tests of the MHD topping cycle will provide the necessary engineering systems data and scaling law verification to permit design and construction of pilot/demonstration plant-sized hardware.

A second government owned, contractor operated test facility is conducting integrated testing of the bottoming cycle of the MHD system, the Heat Recovery and Seed Recovery System (HRSR). The Coal Fired Flow Facility (CFFF) operated by the University of Tennessee Space Institute in Tullahoma Tennessee has successfully operated a 28 MW coal-fired MHD generator and achieved SO_x and NO_x levels well below EPA's new source performance standards. In addition to pollution control, the CFFF has demonstrated the removal of coal ash carried through the generator from the combustor and efficient recovery of the potassium seed for recycling to the MHD generator.

Economics and Environmental Intrusion

Results from comparative analyses of various proposed advanced energy conversion systems have verified the potential attractive use of MHD for coal burning electric utility power generation. Results by General Electric from the well known ECAS(1) and EPRI(2) studies identified open cycle MHD energy conversion systems as the most promising advanced energy systems offering the potential highest efficiency and lowest estimated levelized cost of electricity of all of the advanced coal burning energy systems analyzed by General Electric in those studies. These studies also showed that MHD, in addition to its economic advantage (low cost energy), and conservational advantage (more effective utilization of coal) also offers distinct environmental advantages (less pollutants and heat rejection) compared to conventional technology and other proposed advanced energy conversion technologies.

Summary and Conclusions

Very significant progress was made during the past year in the development of MHD components. A 1000-hour generator test was completed demonstrating that channel electrode lifetimes of up to 8000 hours are possible. This achievement met a requirement of the electric power utility industry for early commercial MHD power plants.

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(1) a. Energy Conversion Alternatives Study--ECAS--General Electric Phase II Final Report, NASA-CR 134949, Vol. II, Part 3, Dec. 1976.

b. "Advanced Power Cycles and Their Potential for Electrical Energy Generation," Fox, G.R., General Electric Company. Proceedings of the American Power Conference, Vol. 40, pp. 464-478 (1978).

(2) "Comparative Study and Evaluation of Advanced Cycle Systems." EPRI AF-664, Project 235-1 Final Report, February 1978.

An MHD coal combustor in which slag is removed has been built and tested and will be used to begin actual coal-fired operation with an MHD channel at the 20 MW level this spring. These tests will provide the data needed to begin 50 MW coal-fired power train testing at the CDIF.

Superconducting magnet technology is well developed and poses no major problems. The heat and seed recovery system and pollution control activity have made important strides this year.

One can therefore conclude with confidence that if properly funded, the technology development now being conducted by DOE's MHD Office will make it possible to begin the commercialization phase of MHD development within the next four to five years.

EXECUTIVE SUMMARY

1000 HOUR MHD ANODE TEST

prepared by
AVCO EVERETT RESEARCH LABORATORY, INC.
a subsidiary of Avco Corporation
2385 Revere Beach Parkway
Everett, Massachusetts 02149

sponsored by
U.S. DEPARTMENT OF ENERGY
Washington, D.C.
under Contract
DE-AC010-80-ET15614

MAY-AUGUST 1981

EXECUTIVE SUMMARY

1000 HOUR MHD ANODE TEST*

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May-August 1981

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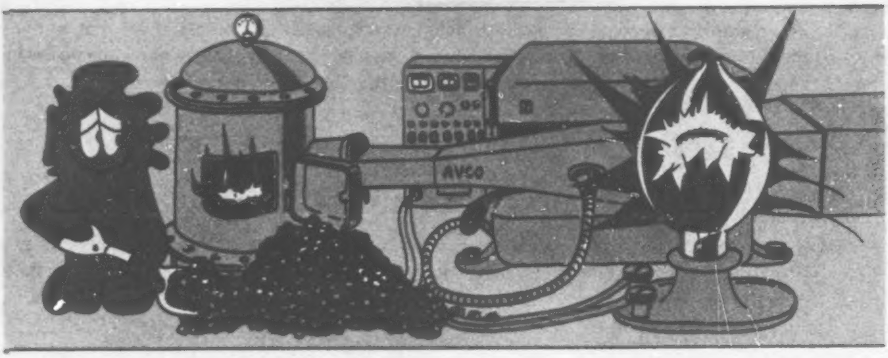
*This work was sponsored by the U.S. Department of Energy under Contract No. DE-AC010-80-ET15614

AVCO EVERETT

MHD



SUMMARY

Various anode electrodes for MHD channels were recently operated successfully for over 1000 hours under the same electrical, thermal and chemical stress conditions as would exist in a commercial MHD power plant, including the requisite fractions of sulfur and coal ash carryover. The most successful electrodes were made of water-cooled copper with platinum and/or stainless steel cladding in critical areas. Based on measured erosion rates for the 1000 hours of operation actually achieved, an ultimate lifetime in the range 5000 to 8000 hours can be extrapolated for these electrodes.

SECTION I
INTRODUCTION

MHD Generator Technology Development to support the National MHD Program sponsored by the U.S. Department of Energy is being carried out under contract by the Avco Everett Research Laboratory, Inc. The goal is commercialization of direct coal-fired, base load MHD power stations in the 1990s.

One of the main objectives of the Avco program has been to develop electrode designs and demonstrate lifetimes of the MHD channel which are adequate to satisfy the requirements of commercial operation. The most difficult technical issue associated with channel lifetime has been the development of electrodes, particularly anodes, with adequate reliability and durability.

Major advances in long duration channel operation have been demonstrated in the past five years. Successful duration tests with the Avco MK VI generator of 100 hours in 1976 and 500 hours in 1978 were key milestones in the national MHD program.

This summary report presents the highlights of a recently completed long-duration test of critical anode electrode designs. The anodes were operated successfully for over 1000 hours under the same electrical and thermal stress conditions as would exist in a commercial power plant. Coal combustion was simulated by adding the requisite fractions of sulfur and coal ash carryover to combustion products of fuel oil and oxygen enriched air.

Accurate measurements of metal erosion rates indicate that lifetimes of several thousands of hours can be confidently predicted for the current "state-of-the-art" electrode designs.

SECTION II
ELECTRODE DESIGN

A reliable long-duration anode has proven to be the MHD channel designer's major challenge. Severe anode corrosion is caused by electrochemical attack of anode surfaces by negatively charged ions of oxygen and SO_X. These ions are liberated from the coal slag by arc current transport through the slag layer, and driven to the anode surface by the electric field. Oxidation and sulfidation of the anodic surfaces with resultant material loss have been major barriers to development of long duration electrodes.

Basic electrode design criteria developed at Avco to meet the demanding requirements of the MHD generator operating environment including the following:

- A massive, well-cooled current leadout of high thermal diffusivity (i.e., metal) to retard electrolytic activity and quench electric arcs;
- Protection of anodic surfaces with a thin cladding of oxidation and sulfidation-resistant material;
- Well-cooled interelectrode insulators with good thermal conductivity to reduce current leakage and/or voltage breakdown;
- A slag-covered surface to protect the metallic electrodes, reduce their operating temperature and minimize heat loss to the cooling water.

Various anode electrode designs meeting these criteria were recently tested for over 1000 hours of operation. Figure 1 is a sketch of the two anode configurations that were most successful. These represent the present state-of-the-art anode design and are made of water-cooled copper, with oxidation and sulfur-resistant cladding attached. Both designs have upstream corners reinforced with platinum because the Hall (axial) field produces current concentrations along this edge. The remainder of the anodic surfaces are clad with platinum foil (configuration #1) and non-nickel bearing stainless steel (configuration #2). The other anodes tested are variations of these two primary designs.

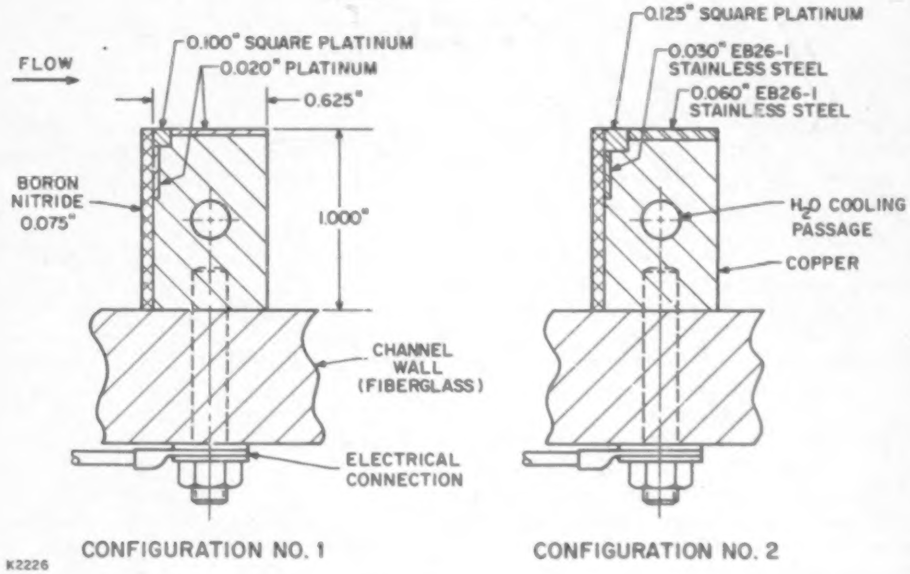

FLOW
0.100" SQUARE PLATINUM
0.020" PLATINUM
0.625"
BORON NITRIDE 0.075"
1.000"
CHANNEL WALL (FIBERGLASS)
ELECTRICAL CONNECTION
CONFIGURATION NO. 1
0.125" SQUARE PLATINUM
0.030" EB26-1 STAINLESS STEEL
0.060" EB26-1 STAINLESS STEEL
H_2O COOLING PASSAGE
COPPER
CONFIGURATION NO. 2
K2226

Figure 1 MHD Anode Geometry - 1000 Hour Test

SECTION III
TEST CONDITIONS

The 1000 hour test was conducted on the Avco Mk VII generator, a 56 electrode pair, 1-meter long channel producing a nominal 55 kW of power. Figure 2 is a photo of the MHD power train. The generator was driven by an oxygen-enriched, ash-injected, oil-fired combustor. Metered quantities of SO_2 were fed into the combustion chamber to provide the equivalent sulfur concentration produced by burning Montana Rosebud coal. Ash recovered from the precipitators of the power station burning Rosebud coal was also injected into the combustion zone to provide the same ash carryover fractions (~25%) to the MHD channel as is predicted for a commercial plant.

Power plant electrical parameters were reproduced in the test channel by:

- Supersonic operation to generate the appropriate Hall field (E_x) with a 2.1 Tesla magnet;
- Individual resistive electrode loading to achieve the required current densities (J_y).

Table 1 lists the channel input flow conditions and Figure 3 is a plot of the 1000 hour averaged current density, Hall field and power density versus electrode number. It is important to note that over the active region of the channel the electrical and thermal stresses on the electrodes were the same as those predicted in a commercial-sized channel. Locations of the two primary anode test configurations (Figure 1) are also shown in Figure 3.

TABLE 1

MHD GENERATOR OPERATING PARAMETERS
1000 HOUR DURATION TEST

Mass Flow:	4#/sec.
Nitrogen/Oxygen:	0.8
Oxygen/Fuel Equivalence Ratio:	0.90 of stoichiometric O_2
Sulfer in Plasma:	0.18 wt%
Ash (Montana Rosebud) Carryover to MHD Channel:	25%
Seed (Dry K_2CO_3):	1.0% (wt) K
Combustor Pressure:	48 psia
Channel Inlet Mach Number:	1.4
Peak Magnetic Field:	2.1 Tesla
Channel Electrical Loading:	Faraday

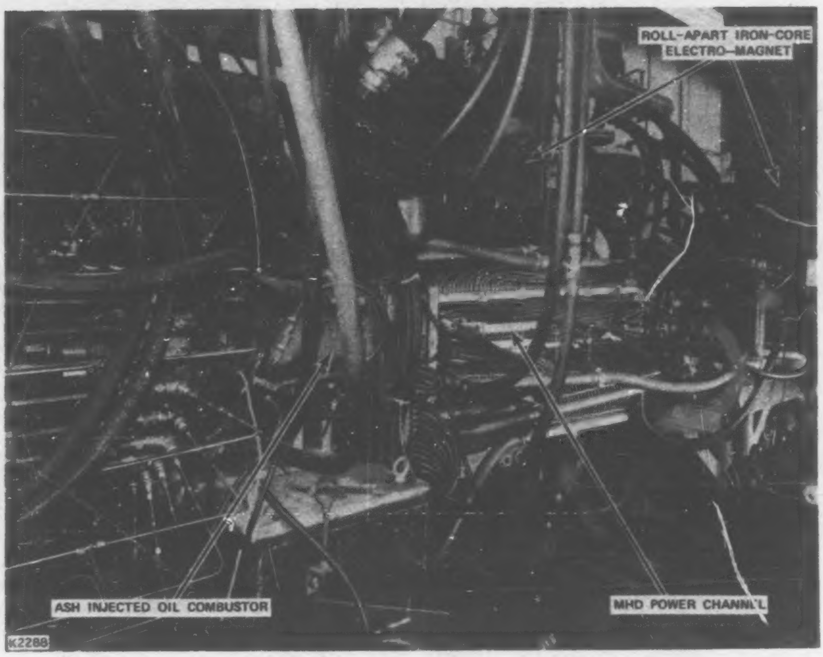

ROLL-APART IRON-CORE ELECTRO-MAGNET
ASH INJECTED OIL COMBUSTOR
MHD POWER CHANNEL
K2288

Figure 2 Avco Mark VII MHD Power Train

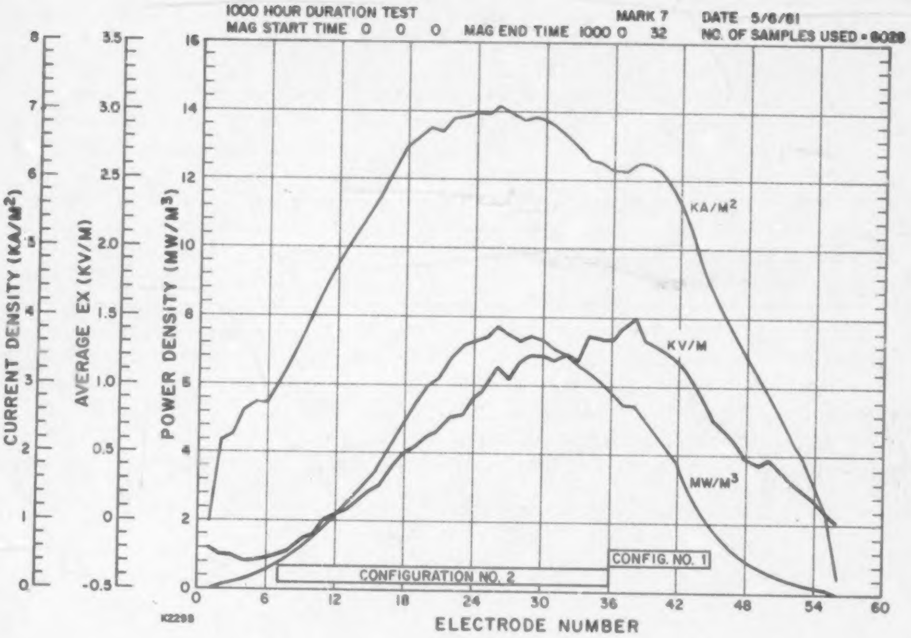


Figure 3 Average MHD Electrical Parameters Generated in 1000 Hour Duration Test

SECTION IV
TEST RESULTS AND DISCUSSION

Upon completion of the test, visual observations and complete photographic records were made of each channel wall. Slag samples were taken for detailed analysis. The anode wall was carefully cleaned of attached slag and disassembled for individual weight measurements and photo documentation. With one minor exception the general appearance of the anodes was excellent. One anode group, in which the stainless caps were attached with a modified brazing cycle, developed a few small local voids in the caps due to inadequate bonding and resultant melting. Although these faults occurred early in the test, the electrodes were not removed. The defect areas did not grow appreciably, however, and these anodes handily completed the 1000 hour test without failure.

Each anode was weighed using a calibrated electronic scale at the beginning of the test, at the 500 hour mark, and at the completion of 1000 power hours. The accuracy of the weight measurement is better than 0.05 gram, and the recorded losses indicate a linear recession rate. Eventually, some of the electrodes will be sectioned to obtain thickness profiles of the protective caps at various stations to determine uniformity of recession.

Figures 4 through 7 are post-test photos of anode designs #1 and #2 after removal of the attached slag layer. Table 2 gives the measured average weight loss and corrosion rate for each of the two design configurations.

TABLE 2

MEASURED ANODE CORROSION, 1000 HOUR TEST

	Average Weight Loss gram/1000 hour	Average Corrosion Rate microgram/coulomb
Configuration #1 (Pt + Pt)	4.27	0.11
Configuration #2 (Pt + SS)	7.71	0.24

Extraploation of the 1000 hour erosion measurements of the platinum clad anode design indicates an *electrode life of approximately 7500 hours.* This is based on a current density of 0.6 amps/cm² and assumes a uniform recession of the metal.

In an optimized 500 MWe baseload MHD-steam power plant the MHD channel would have the following current density profile:

front ⅓: 0.8 to 0.5 amps/cm²
middle ⅓: 0.5 to 0.25 amps/cm²
rear ⅓: 0.25 to 0.10 amps/cm²

Based on the recent test results, the recommended anode configurations to yield a 6000 to 7000 hour life would be:

front ⅓: Pt corner + Pt clad
middle ⅓: Pt corner + SS clad
rear ⅓: SS clad

The total platinum required would be 4500 troy ounce, which represents about 0.1% of the present world annual production.

The platinum cost for the 500 MWe plant would be about $2,000,000 at today's market. This represents only 0.5% of the estimated plant cost and would add at most ½ mill/kWhr to the estimated cost of electricity.

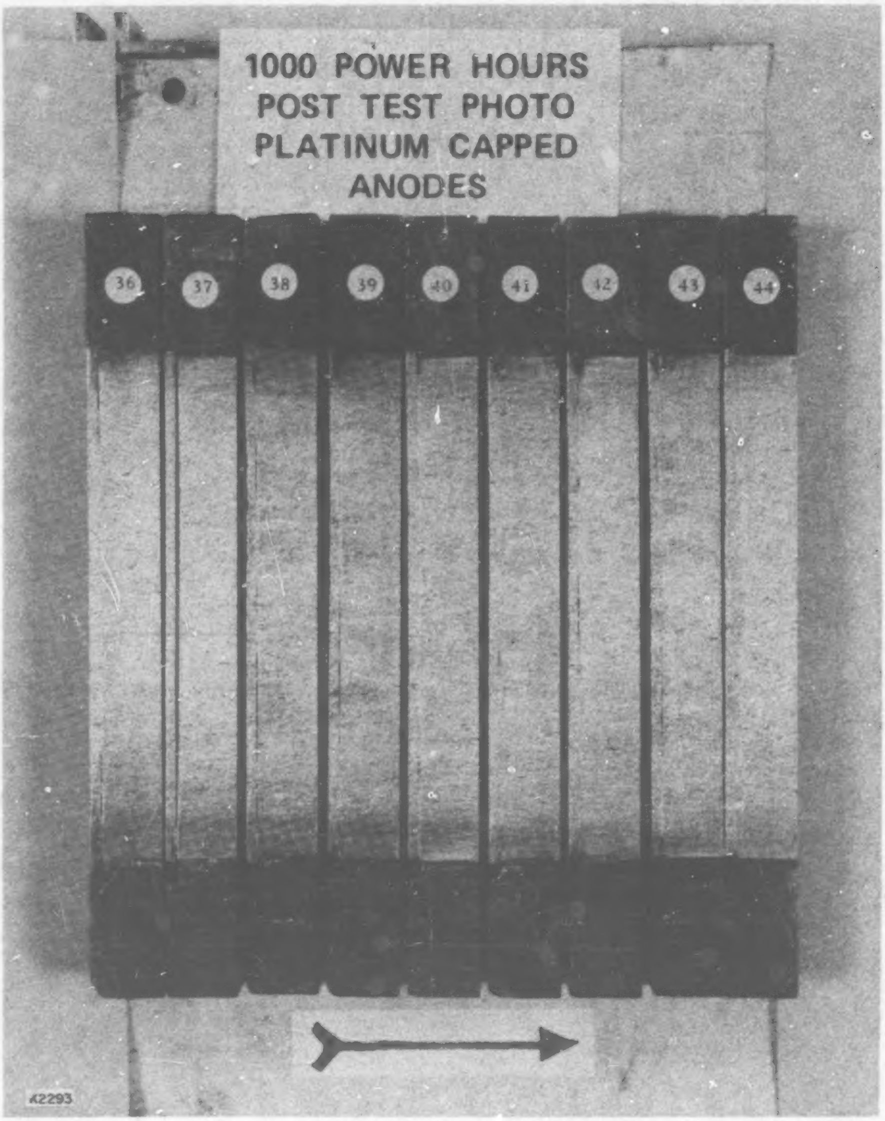

1000 POWER HOURS
POST TEST PHOTO
PLATINUM CAPPED
ANODES
36
37
38
39
40
41
42
43
44
42293

Figure 4 Platinum Clad MHD Anodes After 1000 Hour Test (Design Configuration #1)

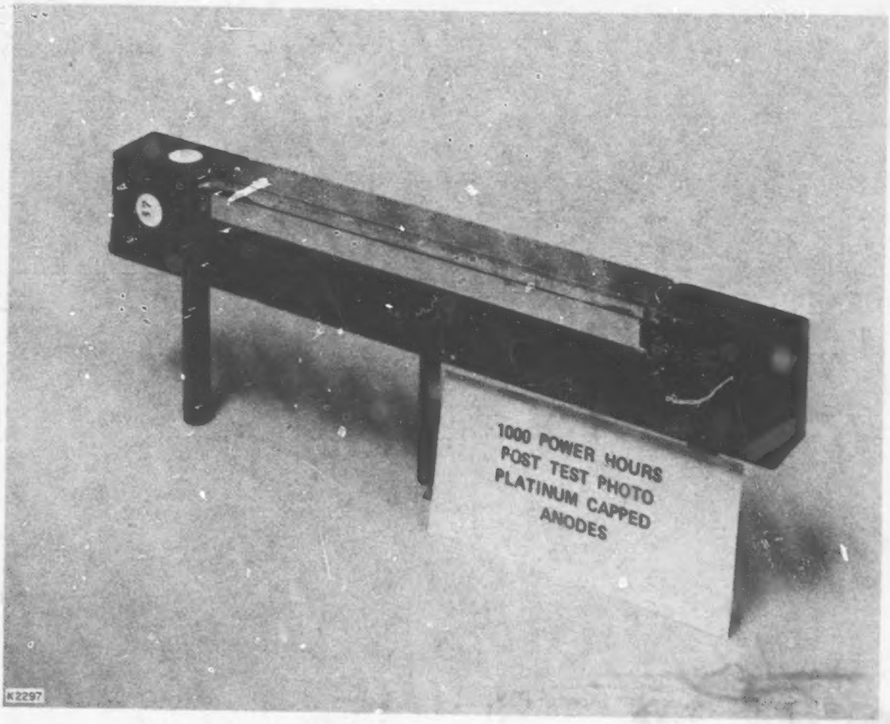

1000 POWER HOURS
POST TEST PHOTO
PLATINUM CAPPED
ANODES
47
K2297

Figure 5 Typical Platinum Clad Anode After 1000 Hour Test (Design Configuration #1)

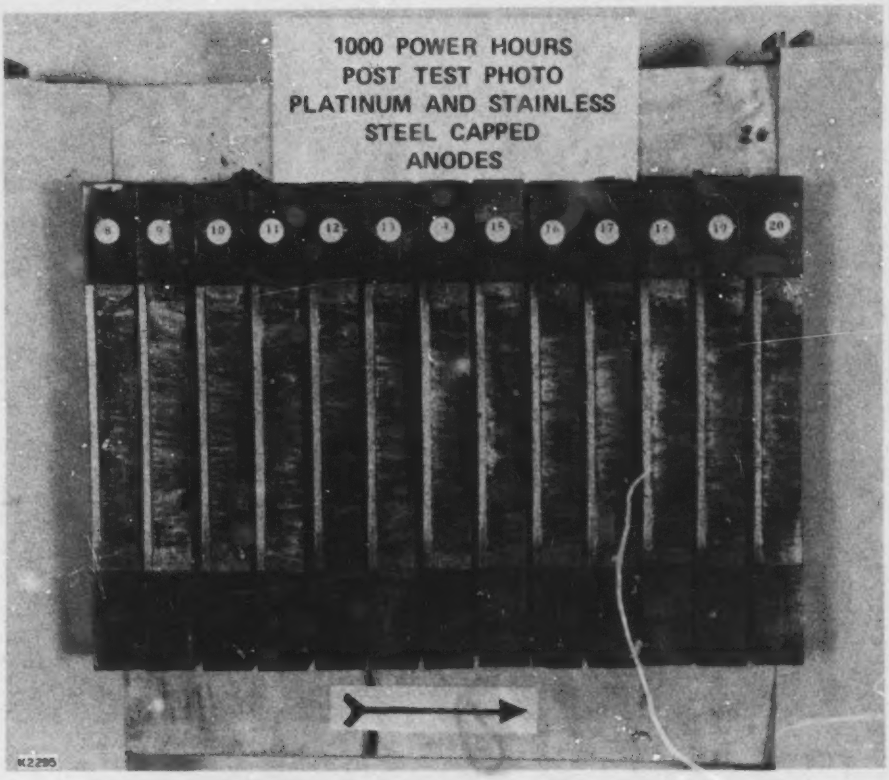

1000 POWER HOURS
POST TEST PHOTO
PLATINUM AND STAINLESS
STEEL CAPPED
ANODES
8
9
10
11
12
13
14
15
16
17
18
19
20
K2295

Figure 6 Platinum and Stainless Steel Clad Anode After 1000 Hour Test (Design Configuration #2)

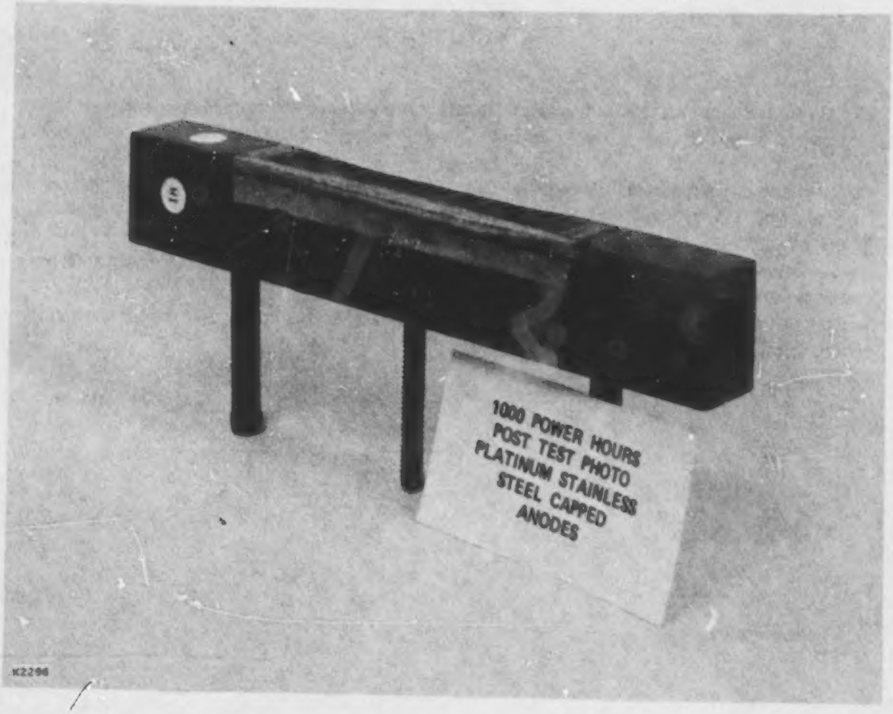

1000 POWER HOURS
POST TEST PHOTO
PLATINUM STAINLESS
STEEL CAPPED
ANODES
K2296

Figure 7 Typical Platinum and Stainless Steel Clad Anode After 1000 Hour Test (Design Configuration #2)

SECTION V
CONCLUSIONS

1. Platinum and platinum/stainless steel clad copper anodes successfully completed a 1000 hour power test in a slagging MHD channel operating under conditions predicted for coalfired commercial power plants.
2. At current densities approaching the design peak in full scale channels the measured platinum corrosion rate was 0.11 microgram/coulomb. The 1000 hour platinum loss per electrode averaged 4.27 grams, of a total platinum weight of 32 grams per electrode.
3. Extrapolation of the lifetime of present state-of-the-art MHD anodes based on the recent 1000 hour demonstration is in the range of 5000 to 8000 hours.
4. The amount of platinum required for commercial baseload MHD channels presents neither a serious penalty nor a problem of availability.

ENVIRONMENTAL INTRUSION OF MHD COMPARED WITH OTHER ADVANCED ENERGY CONVERSION DEVICES

Comparative emission levels of SO_x and NO_x for MHD power plants and other advanced coal burning energy conversion systems are shown in Fig. 1 (attached). Figure 2 (attached) shows comparative total water requirements for the same advanced energy systems. The data is based on results from the well known Energy Conversion Alternatives Study, (ECAS), (1). (The extension of some of the bars shown in Figs. 1 and 2 indicate different values reported for the same advanced energy system.)

The ECAS power plant designs for each of the systems shown assumed use of a high sulphur, Illinois #6 coal. The MHD plant used 1% potassium seed to increase the gas conductivity. The potassium combines with the sulphur to form potassium sulphate which is removed and reused for economic reasons.

Operating with a 1% potassium seed, the ECAS designed MHD power plant effluent would contain 0.5 lb/MBtu of sulphur which is well below EPA emmission requirements. If the seed concentration is increased to 1.2%, all but negligible SO_x will be eliminated from the effluent.

NO_x control in an MHD plant is done internally. By controlling the fuel/air mixture, the temperature in the bottoming plant and the residence time, NO_x levels are reduced to well below EPA standards.

Cooling water requirements are determined by plant efficiency -- higher efficiency, results in less heat rejection and lower cooling water requirements. Consequently, the advanced steam - AFB system showing the lowest efficiency and highest heat rejection has the largest water requirement -- MHD the lowest because of its relatively high efficiency and low heat rejection.

---

(1). Energy Conversion Alternative Study conducted by ERDA, NASA and NSF; November, 1976.

Figure 1

GASEOUS EMISSION CHARACTERISTICS

REFERENCE: ECAS RESULTS (ILL. # 6 BIT. COAL AS FUEL SOURCE)

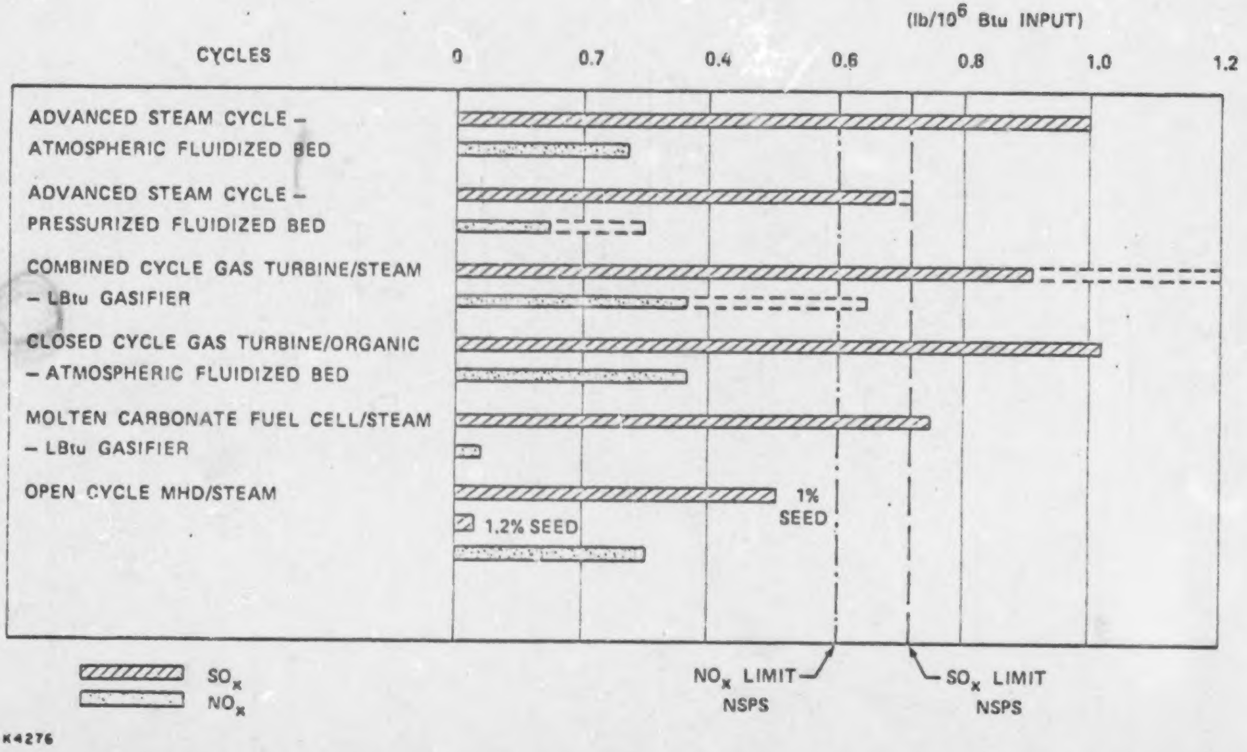
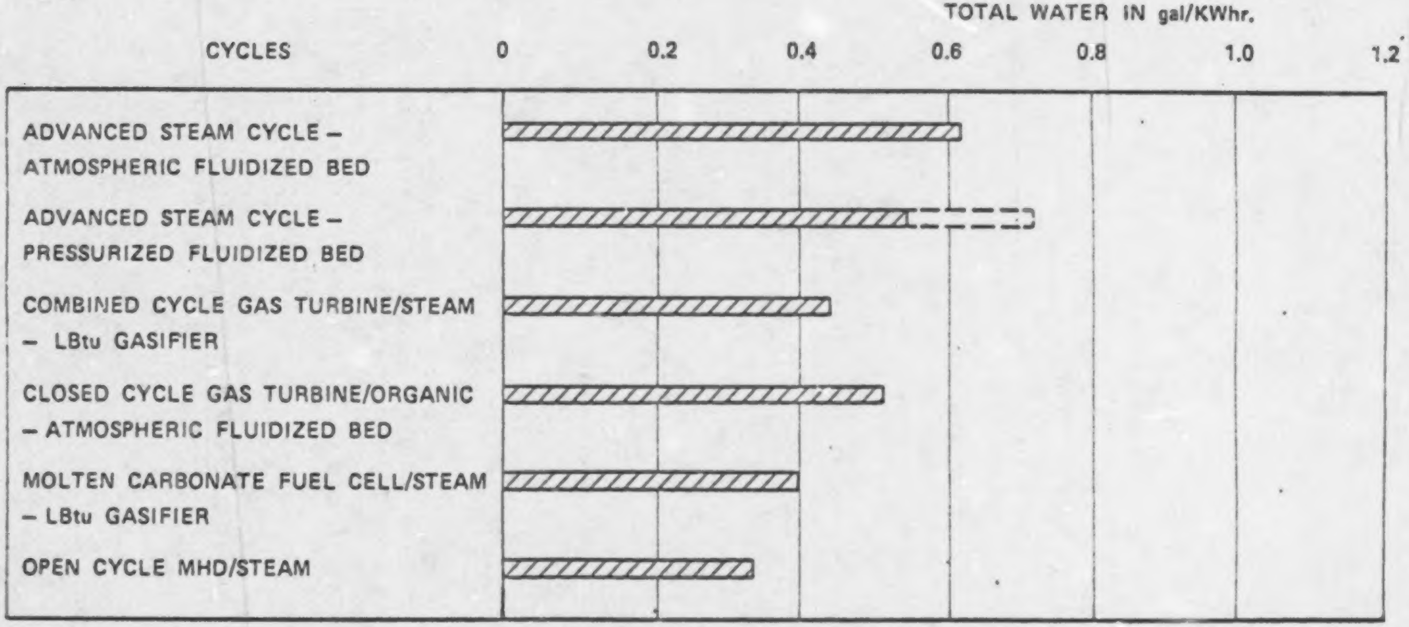


Figure 2

TOTAL, WATER REQUIREMENTS

REFERENCE: ECAS RESULTS (ILL. # 6 BIT. COAL AS FUEL SOURCE)



K4277

TESTIMONY OF
JACK SHERICK
VICE PRESIDENT
AND ASSISTANT GENERAL MANAGER
MOUNTAIN STATES ENERGY, INC.

Mr. Chairman and members of the Committee, my name is Jack Sherick. I am Vice President and Assistant General Manager of Mountain States Energy, Inc. Mountain States Energy is the operating contractor for the Department of Energy's magnetohydrodynamic (MHD) Component Development and Integration Test Facility in Butte, Montana. I am deeply appreciative of the opportunity to submit this testimony in support of adequate funding for the Department of Energy's fossil R & D programs and specifically MHD.

There is a large inconsistency in the Government's approach to the Nation's energy future; energy plays a strategic role in the Nation's economy and defense. A large portion of this Nation's energy needs are being met through use of fossil fuels. In the electric utility industry over 75 percent of the energy used in 1980 was coal, oil or natural gas, with coal providing 49 percent of the total energy used by the electric power indusdry. Projections for the next 20 years show a growing dependence on coal increasing to 69 percent of all electric generation by the year 2000. Yet in 1982 the Administration sought and received over 50 percent reduction of the fossil energy R & D budget and in 1983 is seeking a further reduction of approximately 75 percent.

If this Nation is to maintain its future energy options and minimize its dependence on foreign supplies, we must have a balanced energy R & D program which includes funding for fossil energy research. The major and continuing impact of electric power generation on the economy and environment necessitates that our energy options for the future include advanced coal conversion processes which will be both more efficient and more environmentally acceptable than current coal-fired power plants. MHD is one of the options we believe must be kept open and which we believe fits the Administration's guidelines for Federal support of high-risk and long-term technologies with a high potential payback.

MHD, which when fully developed will offer conversion efficiencies over 50 percent greater than current coal-fired power plants while meeting existing and postulated EPA standards, has application in two major areas of electric power industry. The first is baseload power stations providing either additional new plant capacity or as a replacement for older plants being retired. The second is a retrofit option to existing coal-, oil-, and gas-fired boilers. This retrofit option to existing oil- and gas-fired boilers can both increase efficiency and power output while concurrently converting these power plants to clean-burning coal. In addition to baseload power stations and retrofit to existing boilers, MHD can be used in conjunction with other advanced power conversion technologies such as fusion or in a variety of industrial cogeneration applications. Because MHD is a high temperature fossil fuel technology, research on MHD has also contributed to a variety of developments in high-temperature material science and plasma diagnostics which have had important applications outside of MHD.

During the past decade MHD technology has successfully made the transition from laboratory research to engineering-scale testing. Major advances have been made in virtually all areas of the technology. The problems that must be solved before coal-fired MHD power plants are commercially ready are engineering development problems that can only be addressed through larger-scale testing. Although no major technical breakthroughs are foreseen, demonstration of commercial readiness still requires considerable engineering-scale testing and involves substantial risk.

During the past year, major milestones in technology development have been achieved. Coal combustor development work has demonstrated design feasibility with good performance of critical combustor parameters such as plasma conductivity, slag rejection, heat loss, and high carbon conversion efficiencies. Tests have been conducted at the TRW facility utilizing a 20-megawatt combustor verifying performance and reliability over extended duration. Channel durability and electrode lifetime are critical issues in the development and commercial acceptance of MHD. The recently completed 1,000-hour duration test at AVCO which simulated coal-fired MHD channel operations by injecting fly ash and sulfur into an oil burner extrapolated to a channel lifetime of several thousand hours. Efficient superconducting magnets for use in the MHD process have been successfully demonstrated with the performance of a 40-ton, 5-tesla superconducting magnet shipped to the USSR as part of the cooperative program and more recently by the completion of initial checkout of a 200-ton, 6-tesla magnet built by the Argonne National Laboratory. Preliminary checkout of the 6-tesla magnet showed the magnet exceeded design specification by producing a 6.2 tesla field. In addition, major test facilities in the program have begun producing needed engineering data. These facilities and test data include heat and seed recovery and environmental performance data from the Coal Fired Flow Facility (CFFF) in Tullahoma, Tennessee; high energy extraction performance data from the High Performance Demonstration Facility at the Arnold Engineering Development Center; and 50-megawatt MHD power train performance data from the Component Development and Integration Facility (CDIF) in Butte.

During the past year the CDIF has made the transition from facility activation to an operational test facility. In May 1981 the facility produced its first electrical power. Tests studying combustor and channel performance have been continuing since that time. Recent tests in February of this year achieved power outputs of 1.5 megawatts. Tests scheduled for later this spring will include activation and testing of an EPRI supplied inverter which will allow the MHD produced power to be put onto the commercial power grid. Other major activities in this facility have been concentrating on activation of the coal system in preparation for coal-fired testing some time next fiscal year. The technology is rapidly advancing on all fronts and is now producing much needed engineering data. Remaining though is substantial effort. At CDIF, testing to date has utilized fuel oil and coal ash, rather than coal. The first coal combustor to be tested at this power level has been delayed by this years FY 1982 funding level. The superconducting magnet mentioned earlier is setting idle with no funds to ship or install.

The MHD program in the past has been accused of being too costly. I believe this is in part based upon the original cost estimates for a three-phase program which included a commercial demonstration unit with a total program cost of over $2 billion. In 1981 the Department of Energy revamped this program to a two-phase program, eliminating the costly commercial demonstration unit. Total cost of the program to date has been less than $450 million. It should be noted that this cost includes the total technology development program, not just the component development or partial program development which is typical in other programs and probably represents over half of the total cost of developing the technology to the point where industry can assume the responsibility for commercialization. With the major investment made and the respective test facilities producing needed engineering data, closure of these facilities at this time would not be a cost-effective decision. Within the next few years the viability of this option could be determined. If funding is terminated now it would result in loss of expertise and possible costly restart of the program later. The MHD program needs funding at the $40 million level in FY 1983 if continued advancement is to be possible.

The technology has recently gained considerable support from the utility industry. A number of individual utilities and utility organizations are becoming actively involved in the technology and have submitted testimony to your committee. The Edison Electric Institute recently established a task force on MHD and the Electric Power Research Institute has been participating in the work at the CDIF with the investment of $2.7 million in the CDIF inverter system. In addition, a number of individual utilities are becoming involved in the technology through a project funded through the State of Montana Department of Natural Resources and Conservation (DNRC). The state appropriated $500,000 for development of MHD in the State of Montana. Through this support, the Montana Power Company, using a broad-base of utility participation, is defining the performance requirements for commercializing MHD. Other utility companies are actively studying the feasibility of retrofitting MHD topping cycles to older oil- and gas-fired power plants to increase the plant capacity and efficiency while concurrently converting these power plants to clean-burning coal.

In addition to the United States, several foreign countries have MHD development programs including Japan, Russia, China, Poland, India, and the Netherlands. By far the most ambitious of these are the programs in Japan and the USSR. The Japanese have a 15-megawatt oil-fired test facility at the Electro Technical Laboratory in Takasago. This facility includes both the MHD topping and steam bottoming cycles together in a single facility. The Soviets have a major test facility at the High Temperature Institute in Moscow. This is a 250-megawatt gas-fired plant with both topping and bottoming cycle components utilizing a low-field conventional magnet and has associated with it a smaller 25-megawatt thermal bypass loop utilizing the U.S. supplied superconducting magnet. Furthermore, the USSR is designing a commercial-scale, 500-megawatt electric gas-fired MHD power plant which should be completed by 1986 and is planning a commercial-scale coal-fired unit some time after 1990.

In a recent visit to the United States, a delegation of Japanese experts expressed interest in a cooperative program with the United States which could include an exchange of MHD test articles or direct financial participation of the Japanese in the MHD program in the U.S.

In summary, this country's economic and energy future for the next 30 to 40 years is more dependent on coal as an energy source than any other substance. We have learned through experience in other areas that beside the major loss in the benefits of the R & D, complacency with our R & D efforts results in noncompetitive situations with foreign governments, costly shuffling of money and resources in time of crisis, and a loss in possible spinoffs and technology expansion. For the economic and defense benefits in the country, fossil energy research, specifically coal use research and development, must be increased, not decreased. The positive accomplishments and promise shown by MHD make it a forerunner in this effort.

STATEMENT OF
JOHN J. KEARNEY
SENIOR VICE PRESIDENT
EDISON ELECTRIC INSTITUTE

My name is John J. Kearney and I am a Senior Vice-President of the Edison Electric Institute. EEI is the association of investor-owned electric utilities in the United States and its members provide over 78% of the electric power used in this country.

I am appearing before you today on behalf of the investor-owned electric utilities in support of an increased Fossil Energy Research and Development Program for the Department of Energy, specifically in the area of Magnetohydrodynamics (MHD). Without continued support by DOE of research in coal-fired MHD, the potential use of this technology by electric utilities will be foreclosed for the foreseeable future.

During 1981, electric utilities burned over 590 million tons of coal, or approximately 78% of the coal consumed in this country. By the year 2000, it is estimated that electric utilities will be consuming almost 1.3 billion tons of coal annually. Coal-fired electric powerplants currently provide the majority of our electricity and will continue to do so into the next century.

The technology for using coal to produce electricity today is essentially the same as it was 60 years ago. That technology has reached the limits of its development, with maximum powerplant efficiencies of about 35%.

New technologies for the generation of electricity are needed to get the maximum amount of energy from coal with the least environmental disturbance. MHD is one important approach towards meeting these objectives.

Future combined cycle MHD plants are projected to have a coal pile to power grid efficiency of over 50%. Furthermore, these combined cycle plants have the potential for exceeding current EPA emission requirements without costly flue gas scrubbers.

Some of the areas of MHD technology which have seen recent major accomplishments include:

- generator durability and performance
- coal combustor
- superconducting magnets
- environmental performance
- seed recovery

These program achievements have led researchers to state that no technological breakthroughs are required in the MHD area. What is still needed, however, is further individual component testing leading to the design of an Engineering Test Facility (ETF). The ETF is the vehicle which will provide a demonstration of the commerical capability of MHD.

The utility industry supports MHD development. Several EEI companies have been pioneers in MHD over the past 15 years, most notably Montana Power Company, Southern California Edison Company, Pennsylvania Power and Light and Duke Power Company. In 1980, these utilities, together with six others, formed an MHD Utility Users Group to assist in furthering MHD research. Last year's progress in the MHD program, due in considerable part to the continued support of this Subcommittee, created additional interest in MHD within the utility industry. As a result, the Users Group has now been formally established as an EEI MHD Task Force with 21 members.

As part of their support for the MHD program, and to make certain that the information developed at the various test facilities reflects ultimate user requirements, the EEI Task Force has undertaken a cooperative project to define the minimum acceptable performance requirements for the advanced MHD test system. Under consideration is a second phase of the project, whereby the performance requirements will be expanded into functional and operational requirements for use in the development of a conceptual design for an MHD process demonstration unit.

The State of Montana has financed Phase I, in cooperation with the Montana Power Company. Montana Power and the other utility members of the MHD Task Force are providing substantial manpower to support this effort.

In summary, the utility industry believes that the MHD technology has the potential for greatly improved efficiency in the utilization of coal for the generation of electricity together with reduced environmental impact. As I have indicated, the utility industry is supporting the MHD program; however, without continued DOE participation in the funding of research, the development of coal-fired MHD technology will essentially cease. We believe it is premature to write off such a promising technology at this point.

Thank you for the opportunity to present these comments. I would be pleased to answer any questions you may have.

Statement of Fred E. Walter, Senior Engineer Specialist

Montana Power Company

My statement to this subcommittee is in support of federal funding at a level of $40 million dollars for the Department of Energy, Fossil Fuels Research and Development, particularly Magnetohydrodynamics research.

The Montana Power Company is an electric and natural gas utility serving the western two-thirds of Montana and has subsidiaries which mine coal, in extensive coal holdings in Montana, Wyoming and Texas.

I will direct my comments to needs of the utility industry, the use of coal as a growing energy source, and the potential of the magnetohydrodynamics technology, or MHD, to meet the baseload plant requirements of electric utilities.

It is clear that coal must assume an increasing part of national fuel mix. It is also clear that advanced, efficient, clean burning technologies are needed if coal's proper contribution is to be achieved. This perspective is shared with an ever increasing number of utilities. The Western Utilities Chief Executive Officers unanimously adopted at their January 13, 1982 meeting a resolution and submitted it to the Energy Research Advisory Board of the U.S. Department of Energy. (See Appendix A for the full text and a list of the utility companies involved). This resolution summarizes The Montana Power Company's position.

The United States has not been moving with dispatch in this critical area of coal utilization. In 1974, coal accounted for about 44% of the energy utilities employed to produce electricity. In 1980, that figure had grown by only five percentage points --- to only 49% --- while the consumption of electricity had leaped 19%.

Looking just a few years into the future, the best forecasts indicate the demand for electricity will escalate 114% by the turn of the century, because electricity is that blessedly versatile form of energy that can serve many needs. Coal should and could be used to generate more than two-thirds of the electricity in this country, assuming attractive economics and superior technology. However, the proposed budget shows a substantial reduction in funding of fossil fuels research. Research funding that the utility industry cannot pick up. The utility companies are state regulated entities and there is not the flexibility in the rate structure to support the levels of research required.

Several advanced coal technologies will be needed for the utility industry of the future because of scale, type of load, availability of fuel, and location of plant. Among these advanced technologies are:

1. MHD

2. Fuel cells

3. Hot gas turbines, and

4. Pressurized fluid bed boilers.

All of these are important. However, for baseload, clean-burning coal-fired plants, MHD has the highest potential. Furthermore, MHD due to its cleanness, could be used as a retrofit to present oil fired plants.

Utility interest in MHD is based on two factors: the significant strides which have been made in research, and the imminence with which MHD promises to help meet a four-fold goal the utilities perceive as necessary for their customers. The goal is the evolution of a technology --- or combination of technologies --- which accomplishes these objectives:

- First, to burn coal with high efficiency, but without extensive advanced coal treatment and without gasification.
- Second, to operate with minimal environmental effects and with a reduction in scrubbing and exhaust gas treatment.
- Third, to produce lower-cost electricity - something to ease the pressure of inflation and cost pressures related to increased regulation.
- Finally, to prolong the life of our fossil fuels by materially increasing the efficiency with which we use those fuels.

MHD is the only technology of which I am aware that promises to increase coal combustion efficiency by as much as 50%. To suggest that kind of gain to the utility industry is akin to offering motorists a way to increase gas mileage from 30 miles per gallon to 45 miles per gallon. It is that spectacular...50% increased efficiency.

The fact is that the potential of MHD is being recognized within our country, and elsewhere in the world. In the United States, we have witnessed the formation of the Industrial MHD Forum and the utilities National MHD Users' Group. The Edison Electric Institute (EEI) in recognizing the need for coal options and the potential of MHD has recently incorporated the National MHD Users' Group into its new EEI MHD Task Force. Outside our country, there are active research and development programs for MHD in the Soviet Union, Poland, Yugoslavia, India, China, Netherlands, Sweden and Japan. The Japanese, in addition to their own program, have expressed an active interest in possible participation with the United States in development of MHD. Can all of these nations, as well as many of our own scientists, be wrong about the potential of MHD? I postulate that if we do not pursue MHD research, we may one day be buying this technology from other nations. However, today we are the world leaders in coal burning-MHD technology and in just a few years its viability can be proven.

The raw gain in efficiency is complimented by the ability to meet the second objective of utilities: to operate with minimal environmental effects while reducing the need for treatment of power plant emissions. The MHD process of generating more electricity from each ton of coal will produce a side benefit by removing sulfur from the emissions. Thus, MHD is expected to achieve the twin benefits of reducing pollution and reducing the cost of preserving air quality.

The cost of producing electricity through the MHD process also offers hope for the third objective --- that the beleaguered utility customers may look forward to mitigation of their monthly energy bills. The gain in efficiency will reduce fuel costs and capital investment. The result is that the customer may hope that electricity prices will be held down despite inflation and burgeoning regulatory costs.

The fourth benefit in prospect with commercial operation of MHD is the opportunity to better husband our single abundant non-renewable fuel resource. Coal constitutes roughly 77% of our domestic energy reserves, yet has been furnishing less than 20% of our energy needs.

Yet without MHD efficiency or some unforeseen exotic technology, coal cannot be expected to assume its destiny in our energy mix, nor can we hope to prolong the life of our coal supply.

Whether the promise will be realized in a timely manner is dependent on the National Energy Program and a congressional decision to fund or to abandon funding of the Fossil Fuel and MHD Programs.

Research to date has resolved the major questions about MHD. Nevertheless, the technology still is in the developmental stage. The scale is still too small to show conclusively its full potential to contribute significantly to the nation's energy needs. The risk inherent in moving forward to the point where MHD meets commercial conditions still is too great for the private sector to assume the largest share of the burden.

The MHD program qualifies in all regards for federal funding under the approaches stated in The National Energy Policy Plan, "Securing America's Energy Future," U.S. Department of Energy, July 1981. These criteria are:

-Public spending for long-term, high risk, high payoff technologies.

-Private investment capital unavailable.

-A less expensive alternative to imported oil.

-Generic research which benefits many companies.

Another criterion listed under The National Energy Policy III was that "The governments role is not to select and promote favored sources of energy. Doing so risks wasting the nations' resources." This correlation is even more fitting: The government's role should not eliminate critical sources of energy.

My company has proposed formally to the Department of Energy that the MHD Engineering Test Facility/Commercial Demonstration plant phase of the national program be integrated with a 350 megawatt coal-fired conventional plant scheduled for construction this decade near the City of Great Falls, Montana.

We would provide site development, support facilities, and operate the MHD plant. MPC would contract to buy electricity and steam produced by the federally-owned portion of the plant. Our support, of course, is contingent on the successful resolution of technical problems which will be solved only if the federal government affords the research effort sufficient support. The State of Montana also recognizes the potential of coal fired MHD and has funded The Montana Power Company (through the EEI-MHD Task Force) to define, from the utility industry point of view, the minimum performance requirements for a process demonstration unit. Twenty-one utilities are contributing their time and effort, worth over $100,000 to this project. This effort demonstrates the burgeoning utility interest in MHD and shows a strong desire that the DOE developmental program meet the needs of this vital industry.

I would like to add that as a non-technical person, I have listened to arguments of the proponents and opponents for more than twelve years. It is my observation that the proponents have been much more successful in realizing their claims than the opponents in having their assertions verified.

In summary, I urge adequate federal funding of the Fossil Fuels and MHD Research and Development Programs as a technology which has long-term promise and which transcends both parochial interests and the immediate federal commitment to a broad range of budgetary stringencies. MHD offers our nation, the consumer and the utilities serving them, exceptional opportunities to replace dwindling oil reserves with coal in our total energy mix. We can conserve and enhance our lone abundant energy resource by making the most efficient possible use of coal. We can burn coal in the most environmentally acceptable, efficient manner. We can facilitate the ability of utilities to meet national energy goals, and we can offer the user of electricity hope of genuine progress being made in combating the escalation of the price of that energy form. I believe continued support for MHD is imperative in the context of the national interest.

Thank you for your attention and consideration.

APPENDIX A

MONTANA
POWER
COMPANY
MPC GENERAL OFFICES 40 EAST BROADWAY, BUTTE, MONTANA 59701 • TELEPHONE 406/723-5421

JOSEPH A. McELWAIN
CHAIRMAN OF THE BOARD
AND
CHIEF EXECUTIVE OFFICER

January 22, 1982

Mr. Louis H. Roddis, Chairman
Energy Research Advisory Board
Forrestal Building ER-6
Washington, D. C. 20585

To: ERAB

As Chairman of the Western Utilities CEO Conference, I am transmitting, herewith, a resolution adopted at our meeting January 13, urging continued federal support for the research and development program on magnetohydrodynamics. The member companies of our Conference are listed below. Your consideration will be appreciated.

Sincerely,

Joseph A. McElwain

Joseph A. McElwain

Western CEO Utilities Conference

Arizona Public Service Co.
Idaho Power Company
Montana-Dakota Utilities Co.
Nevada Power Company
Pacific Gas & Electric Co.
Pacific Power & Light Co.
Portland General Electric Co.
Public Service of New Mexico
Public Service of Colorado
Puget Sound Power & Light Co.
San Diego Gas & Electric Co.
Sierra Pacific Power Co.
Southern California Edison Co.
Tucson Electric Power Co.
Utah Power & Light Co.
Washington Water Power Co.

JAM:lw
Enclosure

RESOLUTION

The Western Utilities CEO Conference

The Western Utilities CEO Conference, meeting in regular Session on January 13, 1982, unanimously adopt the following resolution, and instruct the organization's Chairman to promptly convey this resolution to the attention of the members of the Energy Research Advisory Board of the U. S. Department of Energy in the most prominent and urgent manner available:

> "WHEREAS, the Western Utilities CEO Conference is composed of Chief Executive Officers of all the electric utility companies serving the 13 western states, joined together to foster the mutual interests of America's electric power consumers and the electric utility industry, particularly in the western part of the U.S. and,
>
> "WHEREAS, the efficient, flexible and cost effective generation and distribution of electric power is a vital component in the industrial and defense economy and capability of the U. S., and,
>
> "WHEREAS, the generation of electric power in the U. S. is seriously impacted by inflation, environmental restrictions, government regulation, high interest rates, high equipment costs, and a dearth of technology options, and,
>
> "WHEREAS, the use of this nation's immense coal resources for electric power generation -- given appropriate technology for coal's clean and efficient use -- represents the most feasible, long term energy option, and,
>
> "WHEREAS, the Energy Research Advisory Board (ERAB) of the U.S. Department of Energy has recommended the virtual elimination of federal support for Research and Development of coal-using energy conversion technology; therefore,

"BE IT RESOLVED -- that the Western Utilities CEO Conference strongly supports a national commitment to the use of coal for electric power generation in the U. S., as being in the best interests of the nation, electric power consumers, and the electric utility industry. The Western Utilities CEO Conference believes that magnetohydrodynamics represents one of the most efficient methods of utilizing our vast resources of coal for the generation of electric power, with greatly reduced environmental problems. We, therefore, urge ERAB to reconsider its no-funding recommendation on coal energy conversion research, and we recommend in particular that the MHD program receives continued support.

INGAA

JEROME J. McGRATH
President
1660 L Street, N.W.
Washington, D.C. 20036

John H. Cheatham, III
Vice President, General Counsel
and Secretary
1660 L Street, N.W.
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R. E. McKEE
Treasurer
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Houston, Texas 77001

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Owensboro, Kentucky 42301

PETER J. KING, JR.
P.O. Box 1087
Colorado Springs, Colorado 80944

GEORGE L. MORROW
122 South Michigan Avenue
Chicago, Illinois 60603

April 8, 1982

The Honorable Don Fuqua, Chairman
Subcommittee on Energy Development
and Applications
Committee on Science and Technology
Room B-374 Rayburn Building
U.S. House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

Enclosed is a statement on behalf of the Interstate Natural Gas Association of America regarding the proposed Department of Energy FY 1983 budget. We request that this statement be made a part of the record for the hearings held on March 24 or as appropriate.

Thank you.

Yours truly,

Jerome J. McGrath

Jerome J. McGrath
President

JJM:lh
Enclosure

INTERSTATE NATURAL GAS ASSOCIATION OF AMERICA
1660 L STREET, NORTHWEST, WASHINGTON, D.C. 20036, TELEPHONE 202/293-5770

INTERSTATE NATURAL GAS ASSOCIATION OF AMERICA
SUBMITTED TO THE ENERGY DEVELOPMENT
AND APPLICATIONS SUBCOMMITTEE
OF THE
COMMITTEE ON SCIENCE AND TECHNOLOGY
UNITED STATES HOUSE OF REPRESENTATIVES

April 8, 1982

The Interstate Natural Gas Association of America (INGAA) is submitting this statement concerning the proposed Department of Energy FY 1983 Fossil Energy Research and Develpment Program, in particular the federally-funded gas research programs.

INGAA is a non-profit national trade association whose membership consists of virtually all of the major interstate natural gas transmission companies in the United States. INGAA's members account for approximately 90 percent of the natural gas that is transported and sold in interstate commerce. These companies are subject to regulation by the Federal Energy Regulatory Commission (FERC) under the Natural Gas Act (15 USC 717, et seq.) and the Natural Gas Policy Act of 1978 (15 USC 3301, et seq.).

Natural gas constitutes approximately one-fourth of the total energy consumed by our economy. The features of safety, cleanliness, and reliability make gas a much desired fuel. In addition, when burned, gas releases virtually no pollutants and, thus, poses no environmental threat. Ninety-six percent of the gas consumed in this country is produced domestically and, therefore, is invulnerable to foreign embargo. Gas supplies about 40 percent of the total energy requirement of the U. S. industrial sector. As you can see, gas has occupied an essential role in our society. The

INGAA membership is confident that gas can continue to maintain its important place in our energy picture. However, there are certain problems facing the industry which must be resolved.

Until recently, our society and the gas industry had few incentives to investigate the advantages of improved gas technology. For many years, natural gas was largely viewed as a by-product of oil production with whatever needed research being performed by the non-regulated petroleum industry. Prices were held sufficiently low that the utilities had few problems selling gas and much gas was inefficiently utilized. Even if the gas utilities had wanted to improve gas technology, the low rate of return on invested capital allowed by regulatory commissions precluded the financing of technological research. After the 1973 oil crisis and the onset of curtailments in delivered gas along most interstate pipelines, the attitude toward the role of research began to change. In 1974, the Energy Research and Development Administration was established to promote advanced research in all forms of energy including gas. In 1976, the gas industry promoted the concept of an independent research organization to plan, finance, and manage gas-related research and development. That organization is known as the Gas Research Institute (GRI). INGAA completely supports the comments and recommendations of GRI regarding the proposed energy budget.

The gas industry is in the midst of a very critical period which will continue over the next few years in which drastic changes could occur in prices and subsequently in markets. While the industry, the Congress and the Executive Branch will continue to search for possible solutions to pricing and marketing problems, our membership is distressed to see the large reduction in the funds in the FY 1982 budget and the proposed FY 1983 budget for gas-related research and development. The FY 1982 appropriation was $119 million which represented a reduction of $180.8 million from the previous year. For the FY 1983 budget the Administration is suggesting only $19.8 million which is a reduction of almost $100 million from FY 1982.

INGAA supported the Administration's announced policy of increasing reliance on the private sector to fund near-term research while continuing federal funding of long-term and high-risk research. However, now the abrupt and sharp reversal of the proposed federal funding policy will reduce the options for dealing with foreseeable problems in gas supplies and markets and places additional financial burdens on the regulated gas industry and the consumers it serves. The interstate gas transmission companies want to point out that most projections of future gas supplies show a steadily declining volume of conventional, lower forty-eight supply and increasing reliance on supplemental supplies, such as synthetic and unconventional gas. Bringing these supplemental supplies on line will require vastly more research and experience with the various technologies involved than we currently possess. The gas industry through the Gas Research Institute is willing to accept a greater share of R & D funding for these and other projects, but must be given sufficient time to respond. In establishing the Gas Research Institute a deliberate effort was made to coordinate research strategies among various government agencies and the industry. Since the industry has multilayered regulations, federal and state regulatory commissions have an opportunity to review and approve future plans. The period for review and approval takes approximately 15 months. The abrupt withdrawal of substantial federal funding now will have a substantial adverse impact on future research plans. As a regulated industry there are limitations to the amounts that can be directed towards funding short-term and long-term R & D.

The interstate gas pipeline companies believe, therefore, that it is extremely important that federal funding for long-term gas supply research and development programs be continued, and at an amount equal to or greater than the amount appropriated for FY 1982. We recommend and support the funding of long-range gas research in DOE's FY 1983 budget at a minimum level of $104 million.

INGAA recognizes, and appreciates, the pressures on this Committee and and indeed on our government and society brought on by the great budget deficits we are facing in FY 1983 and beyond. Our industry can sympathize with your tasks and support doing whatever is possible to reduce the large amount of our deficit. However, this points out all the more the necessity to set our priorities in proper perspective. What is of more importance to the long-term security of our country than to have the proper technology available to fuel the economy, our defense and consumer needs of our people? If we don't have this energy capability, our other national requirements needing attention may begin to fade in importance.

INGAA appreciates the opportunity to comment on this important matter.

FISCAL YEAR 1983 DEPARTMENT OF ENERGY BUDGET REVIEW

(Fossil Energy and Basic Research)

THURSDAY, MARCH 25, 1982

House of Representatives
Committee on Science and Technology
Subcommittee on Energy Development and Applications,
Washington, D.C.

The subcommittee met, pursuant to notice, at 9:35 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Present: Representatives Bouquard, Volkmer, Winn, Sensenbrenner, Skeen, Schneider, Dunn, and Walker.

Mr. **Fuqua**. The subcommittee will be in order.

This morning we consider the basic research programs in the Department of Energy, for which the administration has requested over $850 million for fiscal year 1983.

The programs include basic energy sciences, university research support, high energy and nuclear physics, and related activities. They support the long-range, frontier science research that is performed at our national laboratories and universities.

I believe that DOE's basic research programs must continue to provide the fundamental scientific and technological base for our energy future. The fact that DOE now ranks third among all Federal agencies in support for basic research underscores the importance of these programs which have become internationally preeminent and a source of national pride.

Furthermore, basic research carried out at our universities not only adds to our knowledge base, but it also helps train the next generation of scientists and engineers who are now in such great demand to work toward the solutions to our energy needs.

The committee has always supported a strong Federal role in basic research. I am pleased that this view is shared by the President's Science Adviser, Dr. Keyworth, and by his predecessors in that position.

However, we now face severe Federal budget constraints, and we must ask the scientific community to make careful choices among research priorities and the allocation of funds. The challenge is to achieve a proper tradeoff between support for research scientists and their experiments, operation of large pieces of laboratory apparatus, and construction of new, more powerful research facilities.

We have three gentlemen who have been involved in one way or another in setting priorities for DOE's research programs.

Our first witness is Dr. Sidney Drell, Deputy Director of the Stanford Linear Accelerator Center, one of the three major U.S. centers for high energy physics research. Dr. Drell is appearing before us as the former Chairman of DOE's High Energy Physics Advisory Panel.

Our second witness is Dr. Herman Feshbach, head of MIT's physics department, one of the leading centers for nuclear physics research. Dr. Feshbach is the present Chairman of DOE's Nuclear Science Advisory Committee.

Our third witness is Dr. Alexander Zucker, associate director of Oak Ridge National Laboratory, which has excellent research programs in basic energy sciences and nuclear physics. Dr. Zucker has recently chaired an ad hoc group that evaluated the quality of DOE's basic energy sciences research.

The Chair would now like to recognize Mr. Winn for an opening statement.

Mr. **WINN**. Thank you, Mr. Chairman. I am also pleased to welcome our distinguished outside witnesses who will comment today on the Department of Energy's budget request in basic research for the fiscal year 1983.

The need to develop new and improved energy resources and to use our energy resources more efficiently is certainly important to the general welfare of our Nation. The programs which we are considering today will provide us with the fundamental knowledge to help us achieve this goal. Funding of basic research is an investment in the future. Failure to strengthen our fundamental knowledge could certainly harm us in the long run.

I am pleased to see that the administration has requested an increase in funding for basic and supporting research for 1983. This action is consistent with the recommendations of the Energy Research Advisory Board, from whom we have recently heard. I strongly support this recommendation. With this funding, I am confident that we will be able to provide the needed support for long-range, high-risk activities for fiscal year 1983.

Thank you again, Mr. Chairman, for allowing me to make these opening remarks. I welcome our witnesses and look forward to their testimony.

Thank you.

Mr. **FUQUA**. Thank you, Mr. Winn.

Dr. Drell, we will proceed with your testimony. We will hear from all of our witnesses and then we will have questions because I think that all of you might want to comment on them, or add, or maybe even subtract—we have had a witness that did that.

We will make your statements, in their entirety, a part of the record. If you wish to summarize we will be pleased for you to do that if you so choose.

Dr. Drell?

[The biographical sketch of Dr. Drell follows:]

Sidney D. Drell
Professional Biography

Present Position

Lewis M. Terman Professor, Deputy Director, and Executive Head Theoretical Physics, Stanford Linear Accelerator Center, Stanford University, Stanford, California

Present Activities

- Member, High Energy Physics Advisory Panel, U.S. Department of Energy (formerly AEC and ERDA), Chairman 1974-82
- Consultant, Senate Select Committee on Intelligence
- Consultant, Office of Science and Technology Policy
- Consultant, Office of Technology Assessment, U.S. Congress
- Consultant, Los Alamos Scientific Laboratory
- Member, Board of Directors, The Arms Control Association, Washington, D. C.
- Member, JASON Division, SRI International (formerly Institute for Defense Analyses) Former Member of JASON Steering Committee
- Member, Board of Trustees, Institute for Advanced Study, Princeton, New Jersey
- Member, Board of Governors, Weizmann Institute of Science, Rehovot, Israel
- Member, Advisory Committee, MIT Physics Department
- Member, Board of Directors, "Annual Reviews, Inc."
- Member, Council on Foreign Relations, New York

Professional and Honorary Societies

- American Physical Society (Fellow)
- National Academy of Sciences
- American Academy of Arts and Sciences
- Phi Beta Kappa
- Sigma Xi
- Phi Kappa Phi

Awards and Honors

- Ernest Orlando Lawrence Memorial Award - 1972 - for research in Theoretical Physics (Atomic Energy Commission)
- University of Illinois Alumni Award for Distinguished Service in Engineering - 1973
- Guggenheim Fellowship - 1961-62 and 1971-72
- Richtmyer Memorial Lecturer to the American Association of Physics Teachers, San Francisco, California - 1978
- Leo Szilard Award for Physics in the Public Interest - 1980 - presented by the Forum on Physics and Science of the American Physical Society
- D.Sc. (Honorary), University of Illinois, Chicago Circle - 1981

Recent Positions and Activities

1978-80	Member, Energy Research Advisory Board, U.S.Department of Energy
1969-76	Member, Editorial Council, "Annals of Physics"
1966-70	President's Science Advisory Committee
1960-73	Office of Science and Technology, Executive Office of the President, Member (or Chairman) of PSAC Panels on national security problems
1974-76	Defense Science Board Task Force
Spring 1975	Schrodinger Visiting Professor, Univ. of Vienna, Austria
Spring 1971	DuPont Lecturer, Univ. of Pennsylvania
Fall 1962, 1970	Visiting Professor and Loeb Lecturer, Harvard Univ.
Winter 1965	AVCO Lecturer, Cornell Univ.
1971-74	Member, Physics Visiting Committee, Harvard Univ.
1967-68	Member, National Accelerator Laboratory Physics Advisory Committee
1966-69	Member, Advisory Council, Physics Dept., Princeton Univ., Chairman 1967-69)
Spring 1979	Visiting Fellow, All Souls College, Oxford Univ.
1969-1981	Consultant, U.S. Arms Control and Disarmament Agency
1973-1981	Consultant, National Security Council

Personal Data

Born September 13, 1926 - Atlantic City, New Jersey
Married (Harriet J. Stainback, Hunter City, Mississippi)
Children (Daniel W., b. 1953; Persis S., b. 1955; Joanna H., b. 1965)

Education

1946	A.B., Princeton University
1947	M.A., University of Illinois
1949	Ph.D., University of Illinois

Special Fields

Theoretical Physics: Elementary Particle Physics & Quantum Theory

Publications

Three books and numerous papers in theoretical physics

STATEMENT OF DR. SIDNEY DRELL, PROFESSOR AND DEPUTY DIRECTOR, STANFORD LINEAR ACCELERATOR CENTER, STANFORD, CALIF., AND FORMER CHAIRMAN, DEPARTMENT OF ENERGY'S HIGH ENERGY PHYSICS ADVISORY PANEL

Dr. **Drell**. Thank you, Mr. Chairman, for the opportunity to appear once again before this subcommittee to discuss the status of the U.S. research program in fundamental particle physics and, as you stated in your letter, "the adequacy of Federal support to maintain the Nation's preeminence in this frontier field."

The High Energy Physics Advisory Panel, or HEPAP, which I have chaired for the past 8 years, has just completed an extensive study of the high energy physics program and analyzed the scientific prospects and the technical status and opportunities before us.

In my testimony I will first describe briefly our current scientific understanding of the basic structure and forces of matter. Next, I will describe both the short- and long-term planning options and problems, including recommendations based on our most recent review.

Finally, I will make some personal comments on the future prospects for the U.S. research program.

Our understanding of the subnuclear world advanced by major strides during the past decade. History will record the 1970's as a spectacularly creative and productive period. We unpeeled another layer of matter in our search for the basic building blocks of nature and have found the so-called quarks.

Quarks are point-like objects which, in groups of three, form the protons and neutrons of which all nuclear matter is composed. Five different kinds of quarks have been identified and a vigorous search is on for a sixth one whose existence has been predicted. Whether the quarks are the fundamental constituents of subnuclear matter, or whether within the quarks themselves we will find yet another layer of matter as we search with even more powerful probes, is an open question for future study.

These same questions can equally well be applied to the electron itself, which has reigned heretofore unchallenged as the elementary unit of electricity. Indeed, there is no direct experimental reason to question the electron's status as an honored member in the social registry of nature's fundamental building blocks. But the decade of the 1970's revealed to us a new particle, just like the electron, but 3,600 times as heavy, the so-called tau lepton. The tau, in fact, is the second heavy brother of the electron; the first one having been discovered 50 years ago. And, if history is an accurate and trusty guide, we should not be surprised to discover at some future time that these three individual members of this family of very similar particles are merely excitations of yet more elementary constituents, much like the excited states of the atom itself; but maybe not.

In our quest for a deeper understanding of nature we ask not only "what are the building blocks", but also "what are the forces that bind or 'glue' the building blocks together?" Our understanding of these forces in the 1970's made a giant leap of progress which history may well prove to rival in importance that great stride taken by Newton in the 17th century when he first deduced the universal character of nature's laws.

Think of the profound influence on our understanding of the universe when Newton first showed that the very same laws that govern how objects move on Earth, also govern the motion of planets around the Sun. Newton first understood the universality of nature's laws. Einstein extended Newton's majestic edifice with his general theory of relativity early in this century and went on to attempt to unify the theory of gravitational phenomena and of electro-magnetic phenomena into one theoretical framework.

Einstein strove for 40 years, until his death in 1955, to accomplish the unification of these two apparently disparate phenomena. It is a measure of the difficulty of the challenge of unification that Einstein failed.

During the past decade, however, we have accomplished a step of comparable importance: the theory of the weak forces responsible for nuclear beta decay has been successfully unified with the theory of electromagnetism.

Furthermore, we now believe that we have the theoretical framework for a further generalization so that the strong nuclear forces can also be unified in a grand structure with the electro-weak theory. Perhaps then, the final unification with gravitational theory and the fulfillment of Einstein's dream, as shared by all scientists, will be achieved.

The 1970's were indeed an extraordinary period for the field. Our new understanding has opened the door toward an understanding of the life cycle of stars and galaxies, of the creation of the chemical elements, and of the very origin of the universe. An exciting voyage in our search for an understanding of the physical universe awaits us in the coming decades. But, as we learned in the first scientific revolution that dates back to Copernicus almost 500 years ago, the first essential ingredient to progress in the study of nature is observation and experimental data. Theoretical prediction is always fallible.

Mr. Chairman, that brings me to a discussion of planning and opportunities, which is the second part of my testimony. The tools that permit us to probe and test on the subnuclear frontiers of today and beyond are necessarily very large, complex, and costly, and they take many years to design, construct, and bring into operation.

Looking ahead to the 1980's and into the 1990's and facing the challenge of maintaining a preeminent national program, the strategic question in U.S. high energy physics is the balance between utilization of existing facilities, the construction of the major facilities for the next stage of research, and the advanced accelerator research and development effort necessary for the long-range progress of the field.

The problem of formulating a successful policy, both for the short term and for the long term, is difficult and complex, and the three major program elements, utilization, construction, and accelerator research and development are coupled to one another in considering specific projects. I refer to planning for the 1980's as short term and for the 1990's as long-term planning.

Let me first focus on the 1980's. The essential program elements for research during this period, as endorsed by HEPAP, are the following:

Adequate utilization and maintenance of existing accelerator and storage ring facilities and support of important nonaccelerator particle physics projects.

Expeditious completion of the energy saver at Fermilab, and in concert with it, implementation of the Tevatron II fixed-target program at Fermilab.

Completion of the Tevatron I proton-antiproton collider at Fermilab, including the provision of a major detector facility.

Continuation of the major R. & D. effort at SLAC on the Stanford linear collider (SLC), with construction of associated conventional facilities to begin in fiscal year 1984 such that the R. & D. phase can be completed in fiscal year 1986.

Finally, pursuit of other advanced accelerator R. & D. activities on items such as high field superconducting magnets, superconducting radio frequency cavities, and novel means of acceleration.

Let me comment briefly on these essential ingredients.

Utilization of present facilities at the Brookhaven National Laboratory, the Fermilab, and the Stanford Linear Accelerator Center, together with the electron-positron storage ring at Cornell which is supported by the National Science Foundation, provides the life blood of the field at this time. These facilities are the source of the vital new data. They provide support for researchers from more than 100 institutions in every geographical region of the United States.

It is essential to attract, to train, and to retain outstanding young scientists in this exacting field of research if the U.S. high energy program is to remain preeminent. They are no less vital than the facilities themselves. Adequate research opportunities must be available both to advance the research frontiers and to develop highly talented new research leaders.

Along with adequate utilization, HEPAP assigns highest immediate priority to the expeditious completion of the Fermilab Energy Saver. Its construction is well underway, and operational trials are scheduled for 1983. Not only will this device be the world's first superconducting synchrotron, and in fact, the first major superconducting facility of any kind, which ranks it as a major technological advance. It is also the essential component for completing the Tevatron facilities at Fermilab which will operate at the world's highest energy throughout the 1980's.

Tevatron I is a storage ring that will provide proton-antiproton collisions at 2,000 Gev or 2 Tev. Tevatron II will provide beams of various subnuclear particles impinging on fixed targets with energies of up to 1 Tev. These facilities will be the backbone of the university user program. They will also open broad, new regions of physics.

One other aspect of the energy saver that is worth mentioning is its economy in electric power consumption as a result of operating with the new superconducting technology.

The Stanford Linear Collider represents a dramatic advance in the field of particle accelerators. As an accelerator research project, it provides the means for a thorough investigation and demonstration of a promising new technology for colliding micron-size electron and positron beams. This is viewed as a crucial step in the design of very high energy electron-positron colliders for the future. At the same time, the Stanford linear collider offers the opportunity to do very exciting new physics in a timely fashion. In particular, we expect to be able to explore the massive quantum of the unified electro-weak interactions—the so called Z_0 quantum—as predicted by present day theory.

HEPAP attaches great importance to the Stanford linear collider, both for developing accelerator technology and for studying new physics, and recommends continued R. & D. leading to construction starting in fiscal year 1984. This device is an example of the type of relatively large R. & D. project which we now view as necessary in the development of new accelerator systems.

Beyond the SLC, advanced accelerator R. & D. is an essential ingredient of the program if the high energy frontier is to continue to advance. It is essential that we continue to invest in accelerator R. & D. so that, by our wits, we can develop more powerful tools and techniques within reasonable budgets; that is, by decreasing the costs per Gev of colliding energy.

What I have described in these elements, the Tevatron, the energy saver, utilization, the collider and accelerator R. & D., are essential elements of a program that will be healthy, vital, and competitive for the decade of the 1980's. HEPAP has endorsed the conclusion that this program can be implemented within the President's budget request for fiscal year 1983 of $429 million in Department of Energy support. This budget level will restore the program back almost to the 1981 operating level, and thereby stop the continuing harmful decline of support and activity in our field. This is shown in figure 1 at the back of my testimony, where in 1983 dollars, the total budget is seen now to rise back almost to the 1981 level with the President's budget, and to stop a decline that has been in progress for the last 4 years.

This budget level is essential as a planning floor in the 1980's if we are, to use the chairman's words, "to maintain the Nation's preeminence in this frontier field." It is a lean budget providing little more than one-half the level of support now being devoted to this field by the Western European community, primarily through their international collaboration at the Center for Nuclear Research in Geneva and at the German Synchrotron Laboratory in Hamburg. This figure is shown in figure 2 in my written testimony which gives the funding in percentage of the GNP—it is also approximately in dollars—for the European collaboration and for United States, starting in 1966. Notice we started at the same level in 1966, but the United States is now down to roughly 70 percent relative to the European investment. The Japanese, it is interesting to note, have now started a major construction program and are rapidly approaching the U.S. level of support.

With our existing strength and base of operations, we are confident that the program which I have described and which can be supported at the recommended budget level will flourish in the 1980's and can provide a base for entering the 1990's in spite of that funding gap.

However, if there are further cuts, program elements essential to our short-term planning will have to be dropped or delayed: Reduced utilization, delayed completion of the Tevatron, or reduced accelerator R. & D. and a delayed linear collider at SLAC will cause serious losses to the U.S. high energy program. Such measures will not be consistent with the goal of maintaining a strong national effort in this field in the decade ahead stated by the administration in its budget.

This program that I have described, Mr. Chairman, is not without serious difficulties when it comes to long-range planning for the 1990's. It was a basic conclusion of the recent HEPAP review that it is essential for an active and healthy U.S. program in the 1990's that a major new facility for experimentation be available by the end of this decade. In our current plans the Isabelle project at Brookhaven National Laboratory is envisaged as this facility. When completed, it can provide a unique scientific capability that combines very high energy and very high luminosity. High luminosity means a high rate of interaction by the two colliding beams of particles. This is necessary both for high statistics studies involving the new processes and particles already predicted for this energy region, as well as for searches for rare processes and strange phenomena that are undoubtedly lurking there to surprise us.

Isabelle not only has a very high potential for exploratory work but, as a facility, has excellent growth potential. It will provide experimental opportunities complementing the electron-positron colliders and the Tevatron project. It also will be vital for supporting the university user base of the U.S. program in the 1990's.

In view of its importance to the long-range future of the U.S. high energy research program, HEPAP recommends moving ahead with construction of Isabelle at Brookhaven, with completion by the end of the decade.

Recent successes in the superconducting magnet development at Brookhaven have greatly increased technical confidence in the current accelerator design. The major obstacle at this time appears to be the prospect for adequate funding.

We have made an extensive analysis of the budget level required for the U.S. program in order to encompass the Isabelle construction as part of our long-range planning while, at the same time, meeting the short-term program goals for the 1980's as I have just detailed. Such a program requires an annual support which rises to approximately $480 million, in fiscal year 1983 dollars, from the Department of Energy by fiscal year 1984 or 1985 at the latest. This budget level is close to that given as the planning floor—there is a dotted line shown in figure 1—in the OMB–DOE agreement of 1978. It was under this agreement, which was identified as the minimum funding level that construction of Isabelle and the Tevatron project was initiated. The importance of that program and of this budget level was reaffirmed strongly in the HEPAP review.

We recognize, however, that the necessary funding level for continuing the present Isabelle project is beyond the present planning for the high energy physics program. Therefore, we have reviewed how best to structure a national program toward meeting both our short-term and long-term planning goals at the level currently proposed in the President's budget for fiscal year 1983, namely the $429 million figure which is about 10 percent lower than the required level.

HEPAP has concluded that if the U.S. high energy program continues to operate at the President's proposed budget level the present Isabelle project cannot continue. However, there still remains the compelling need for a major facility to be started in the mid-1980's so as to be available for experimentation by around 1990. This facility would have to be less expensive than Isabelle but must be capable of supporting a broad physics program and of exploring new frontiers. The proposed $429 million level of the President's budget provides for the essential accelerator R. & D. during the coming year for work to continue toward this vital long-term planning goal.

In summary, I emphasize the importance of maintaining the President's proposed budget for fiscal year 1983. It will support a lean but a balanced U.S. high energy physics program that can meet the short-term goal of maintaining preeminence in the 1980's and simultaneously lay the necessary groundwork for its long-term future in the 1990's.

To conclude, Mr. Chairman, I want to speak very briefly, not as a practicing high energy physicist or as ex-Chairman of HEPAP concerned with planning, effectiveness, and wisdom of our planning for the future program. I want to speak simply as a citizen concerned about the future of this great Nation.

There can be no doubt that over the long haul the basic research and educational programs are vital investments in our country's future. They are crucial to the kind of society we are and, beyond that, they are the foundations for an advanced technological enterprise that has been a great strength and source of prestige for the United States.

Yet, looking back, we find that support of basic research in this country has dropped by 21 percent, as a portion of the gross national product, from a figure of 0.38 percent in the peak years of fiscal year 1967 and 1968 to the 1981 level of 0.30 percent. In the brief 2 years from fiscal year 1980 to 1982, total nondefense Federal R. & D. has declined in real dollars by 16.1 percent, basic research by 5 percent, and nondefense basic research by 6.6 percent. Figure 2 shows that the drop in high energy physics has even been more precipitous. High energy physics is a very basic research frontier, far removed from immediate practical applications and supported solely by annual Federal funding. If these downward trends continue, the basic research effort of this country, already in difficulty, will be further jeopardized.

History records that Europe led the world in science and learning and also in technology for almost 500 years, from the time of the Renaissance and the birth of modern science with Copernicus. Europe's baton of leadership passed to the United States as a result of two devastating world wars and the flight to our shores of many leading scholars escaping from Nazi persecution. It saddens me greatly to see this Nation giving that baton back without cause or reason after possessing it a mere 50 years. Yet, this is the inevitable consequence of a continued decrease in our investment in basic research and learning.

To allow such a decline to continue will be a tragically shortsighted vision of the Nation's future. I fervently urge our leadership in the Congress and the administration to prevent this from happening.

[The prepared statement of Dr. Drell follows:]

TESTIMONY ON THE HIGH ENERGY PHYSICS PROGRAM IN THE DEPARTMENT OF ENERGY AND THE FY 1983 BUDGET

by

SIDNEY D. DRELL

before

THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
OF THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

March 25, 1982

Thank you for the opportunity to appear once again before this subcommittee to discuss the status of the U.S. research program in fundamental particle physics and "the adequacy of federal support to maintain the nation's preeminence in this frontier field." The High Energy Physics Advisory Panel, or HEPAP, which I have chaired for the past eight years, has just completed an extensive study[1] of the U.S. high energy physics program and analyzed the scientific prospects and the technical status and opportunities before us.

My testimony today is divided into three parts. First I will describe briefly our current scientific understanding of the basic structure and forces of matter. Next I will describe both the short-term and long-term planning options and problems, including recommendations based on our most recent review. Finally, I will offer my personal views of the future prospects for the U.S. to maintain our current "preeminence in this frontier field."

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[1] Carried out by a HEPAP Subpanel on Long Range Planning chaired by Dr. George Trilling of the University of California at Berkeley.

STATUS OF THE FIELD

Our understanding of the subnuclear world advanced by major strides during the past decade. History will record the 1970's as a spectacularly productive and creative period. We unpeeled another layer of matter in our search for the basic building blocks of nature and have found the so-called quarks. Quarks are point-like objects which, in groups of three, form the protons and neutrons of which all nuclear matter is composed. Five different kinds of quarks have been identified and a vigorous search is on for a sixth one whose existence has been predicted. Whether the quarks are the fundamental constituents of subnuclear matter or whether within the quarks themselves we will find yet another layer of matter as we search with even more powerful probes is an open question for future study.

These questions can equally well be applied to the electron itself, which has reigned heretofore unchallenged as the elementary unit of electricity. Indeed, there is no direct experimental reason to question the electron's status as an honored member in the social registry of nature's fundamental building blocks. But the decade of the 1970's revealed to us a new particle thirty-six hundred times as heavy as the electron (the "tau" lepton). Aside from its large mass the tau seems very much like an electron, into which it decays along with two neutrinos in little more than two ten-thousandths of a billionth of a second. It is, in fact, the second heavy brother of the electron, the first one being the mu lepton discovered almost fifty years ago. If history is an accurate or trusty guide, we should not be surprised if we discover at some future time that the individual members of this family of very similar particles are merely excitations of yet more elementary constituents, much like the excited states of the atom itself. But maybe not.

In our quest for a deeper understanding of nature, we ask not only "what are the building blocks?", but also "what are the forces that bind or 'glue' the building blocks together?". Our understanding of these forces in the '70's made a giant leap of progress which history may well prove to rival in importance that great stride taken by Newton in the 17th century when he first deduced the universal character of nature's laws. Think of the profound influence on our understanding of the universe when Newton first showed that the very same laws that govern how objects move on earth, such as a pencil falling from the table top, also govern the motion of the planets around the sun! Newton first understood the universality of nature's laws. Einstein extended Newton's majestic edifice with his general theory of relativity early in this century and went on to attempt to unify the theory of gravitational phenomena and of electromagnetic phenomena into one theoretical framework. Einstein strove for forty years, until his death in 1955, to accomplish the unification of these two apparently disparate phenomena. It is a measure of the difficulty of the challenge of unification that Einstein failed. During the past decade, however, we have accomplished a step of comparable importance: the theory of the weak forces responsible for nuclear beta decay has been successfully unified with the theory of electromagnetism. Furthermore, we now believe that this revolutionary achievement of the 1970's has provided us with the theoretical framework for a further generalization so that the strong nuclear forces can also be unified in a grand structure with the electro-weak theory. Perhaps then the final unification with gravitational theory and the fulfillment of Einstein's dream, as shared by all scientists, will be achieved.

The 1970's were indeed an extraordinary period for the field. Our new understanding has opened the door toward an understanding of the life cycle of stars and galaxies, of the creation of the chemical elements, and of the very origin of the universe. An exciting voyage in our search for an understanding of the physical universe awaits us in the coming decades. But, as we learned in the first scientific revolution that dates back to Copernicus almost 500 years ago, the first essential ingredient to progress in the study of nature is observation and experimental data. Theoretical predic[illegible] is always fallible! Mr. Chairman, that brings me to a discussion of planning and opportunities, which is the the second part of my testimony. The tools that permit us to probe and test on the subnuclear frontiers of today and beyond are necessarily very large, complex, and costly, and they take many years to design, construct, and bring into operation.

PLANNING FOR THE FUTURE

Looking ahead to the 1980's and into the 1990's and facing the challenge of maintaining a preeminent national program, the strategic question in U.S. high energy physics is the balance between utilization of existing facilities, the construction of the major facilities for the next stage of research, and the advanced accelerator research and development effort necessary for the the long-range progress of the field. The problem of formulating a successful policy, both for the short term and for the long term, is difficult and complex, and the three major program elements mentioned above are coupled to one another in considering specific projects. I refer to planning for the 1980's as "short term" and into the 1990's as "long term."

Let me focus first on the 1980's. The essential program elements for research during this period, as endorsed by HEPAP, are the following:

Adequate utilization and maintenance of existing accelerator and storage ring facilities and support of important non-accelerator particle physics projects.

Expeditious completion of the Energy Saver at Fermilab.

Implementation of the Tevatron II (Tev II) fixed-target program at Fermilab.

Completion of the Tevatron I (Tev I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

Utilization of present facilities at the Brookhaven National Laboratory, the Fermilab, and the Stanford Linear Accelerator Center, together with the electron-positron storage ring at Cornell University (which is supported by the National Science Foundation) provides the life blood of the field at this time. These facilities are the source of the vital new data. They provide support for researchers from more than one hundred institutions in every geographical region of the United States. It is essential to attract, to train, and to retain outstanding young scientists in this exacting field of research if the U.S. high energy program is to remain preeminent. They are no less vital than the facilities themselves. Adequate research opportunities must be available both to advance the research frontiers and to develop highly talented new research leaders.

Along with adequate utilization, HEPAP assigns highest immediate priority to the expeditious completion of the Fermilab Energy Saver. Its construction is well under way, and operational trials are scheduled for 1983. Not only will this device be the world's first superconducting synchrotron, and, in fact, the first major superconducting facility of any kind, which ranks it as a major technological advance; it is also the essential component for completing the Tevatron facilities at Fermilab which will operate at the world's highest energy throughout the 1980's. Tevatron I is a storage ring that will provide proton-antiprotron collisions at 2 TeV, and Tevatron II will provide beams of various subnuclear particles impinging on fixed targets with energies of up to 1 Tev. These facilities will be the backbone of the university user program. They will also open new regions of physics and enable us to explore broadly as we navigate in our voyage of discovery to new surprises and to deeper understanding. One other aspect of the Energy Saver that is worth mentioning is its economy in electric power consumption as a result of operating with the new superconducting technology.

The Stanford Linear Collider (SLC) represents a dramatic advance in the field of particle accelerators. As an accelerator research project, it provides the means for a thorough investigation and demonstration of a promising new technology for colliding micron-size electron and positron beams. This is viewed as a crucial step in the design of very high energy electron-positron colliders for the future. At the same time, the SLC offers the opportunity to do very exciting new physics in a timely fashion. In particular, we expect to be able to explore the massive quantum of the unified electro-weak interactions - called the Z_0 - as predicted by present-day theory. HEPAP attaches great importance to the SLC, both for developing accelerator technology and for studying new physics, and recommends continued R&D leading to construction starting in FY 1984. The SLC is an example of the type of relatively large R&D project which we now view as necessary in the development of new accelerator systems.

Beyond the SLC, advanced accelerator R&D is essential if the high energy frontier is to continue to advance. During the past forty years developments in the technology of accelerators and storage ring facilities have enabled us to increase collision energies by a factor of ten million, while at the same time reducing the cost per unit of energy by a factor of a million. It is essential that we continue to invest in accelerator R&D so that, by our wits, we can develop more powerful tools and techniques within reasonable budgets.

What I have described thus far are the essential elements of a program that will be healthy, vital, and competitive for the decade of the 1980's. HEPAP had endorsed the conclusion that this program can be implemented within the President's budget request for FY 1983 of $429M in Department of Energy support (plus an anticipated support of approximately $34M by the National Science Foundation). This budget level will restore the program back almost to the 1981 operating level and thereby stop the continuing harmful decline of support and activity in our field (See Figure 1). This budget level is essential as a planning floor in the 1980's if we are "to maintain the nation's preeminence in this frontier field." It is a lean budget providing little more than one-half the level of support now being devoted to this field by the Western European community, primarily through their international collaboration at the Center for Nuclear Research in Geneva and at the German Synchrotron Laboratory in Hamburg (See Figure 2.) With our existing strength and base of operations, we are confident however that the program which I have described and which can be supported at the recommended budget level will flourish in the 1980's and can provide a base for entering the 1990's.

In its analysis of the FY 1983 budget (Special Analysis K), the Office of Management and Budget stated:

> "Because of the exceptional research opportunities in high energy physics and the demonstrated excellence of the U.S. program, the Administration is committed to maintaining strong national effort in this field."

I believe that we can meet this committment during the coming decade with a program budget at the level requested for FY 1983. However, if there are further cuts, program elements that are essential to our short-term planning will have to be dropped or delayed. Reduced utilization, delayed completion of the Tevatron program or reduced accelerator R&D and a delayed SLC will cause serious losses to the

U.S. high energy program. Such measures will not be consistent with the goal of "maintaining a strong national effort in this field" in the decade ahead.--

The program I have described is not without serious difficulties when it comes to "long-range" planning for the 1990's. It was a basic conclusion of the recent HEPAP review that it is essential for an active and healthy U.S. program in the 1990's that a major new facility for experimentation be available by the end of this decade. In our current plans the ISABELLE project at the Brookhaven National Laboratory is envisaged as this facility. When completed, it can provide a unique scientific capability that combines very high energy (an 800 GeV interaction energy for the two colliding proton beams) and very high luminosity. High luminosity means a high rate of interaction by the two colliding beams of particles. This is necessary both for high statistics studies involving the new processes and particles already predicted for this energy region, as well as for searches for rare processes and strange phenomena that are undoubtedly lurking there to surprise us. ISABELLE not only has a very high potential for exploratory work but, as a facility, has excellent growth potential. It will provide experimental opportunities complementing the electron- positron colliders (such as SLC) and the Tevatron project. It also will be vital for supporting the university user base of the United States program in the 1990's. In view of its importance to the long range future of the U.S. high energy research program, HEPAP recommends moving ahead with construction of ISABELLE at Brookhaven, with completion by the end of the decade. Recent successes in the superconducting magnet development program at Brookhaven have greatly increased technical confidence in the current accelerator design. The major obstacle at this time appears to be the prospect for adequate funding.

HEPAP has made an extensive analysis of the budget level required for the U.S. program in order to encompass the ISABELLE construction as part of our long-range planning while at the same time meeting the short-term program goals for the 1980's, as detailed above. Such a program requires an annual support which rises to approximately $480M (in FY83 dollars) from DOE, plus $38M from the NSF by fiscal year 1984, or 1985 at the latest, if the ISABELLE project is to be completed in a timely fashion. This budget level is close to that given as a planning floor in the 1978 agreement between the OMB and DOE when the minimum funding level was identified under which construction of the ISABELLE and of the TEVATRON project could be initiated. The importance of that program and the budget level was once again reaffirmed by the HEPAP review.

We recognize that the necessary funding level for continuing the present ISABELLE project is beyond the present planning for the high energy program. Therefore,we have reviewed how best to structure a national program toward meeting both our short-term and long-term planning goals at the level currently proposed in the President's budget for FY 1983. HEPAP has concluded that if the U.S. high energy program continues to operate at the President's proposed budget level the present ISABELLE project cannot continue. However, there still remains the compelling need for a major facility to be started in the mid-1980's so as to be available for experimentation by around 1990. This facility would have to be less expensive than ISABELLE but must be capable of supporting a broad physics program and of exploring new frontiers. The proposed $429M level of the President's budget provides for the essential accelerator R&D during the coming year for work to continue toward this vital "long-term" planning goal.

In summary, I emphasize the importance of maintaining the President's proposed budget for FY1983. It will support a lean but balanced U.S. high energy physics program that can meet the short term goal of maintaining preeminence in the 1980's and simultaneously lay the necessary groundwork for its long term future in the 1990's.

CONCLUSION

In conclusion, Mr. Chairman, I want to set aside my enthusiasm as a high energy physicist for this exciting field of research that is to me so compelling in its fascination. I also want to step out of my role as chairman of HEPAP for the past eight years, during which I have been heavily concerned with the effectiveness and wisdom of our planning for the future of the program. I want to speak simply as a citizen concerned about the future of this great nation.

There can be no doubt that over the long haul the basic research and educational programs are vital investments in our country's future. They are crucial to the kind of society we are, and beyond that they are the foundations for an advanced technological enterprise that has been a great strength and source of prestige for the United States. Yet looking back we find that support of basic research in this country has dropped by 21% as a portion of the gross national product[2] from 0.38% in the peak years of FY 1967 and FY 1968 to the FY 1981 level of 0.30%. In the brief two years from FY 1980 to FY 1982 total non-defense federal R&D declined[3] in real dollars by 16.1%, and basic research by 5%, and non-defense basic research by 6.6%. Figure 2 shows that for high energy physics, a very basic research frontier far removed from immediate practial applications and supported solely by annual federal funding, the decline has been even more dramatic. If these downward trends continue the basic research effort of this country, already in difficulty, will be further jeopardized.

History records that Europe led the world in science and learning - and also technology - for almost 500 years from the time of the Renaissance and the birth of modern science with Copernicus. Europe's baton of leadership passed to the U.S. as a result of two devastating world wars and the flight to our shores of many leading scholars escaping from Nazi persecution. It saddens me greatly to see this nation giving that baton back without cause or reason after possessing it a mere fifty years. Yet this is the inevitable consequence of a continued decrease in our investment in basic research and learning. To allow such a decline to continue will be a tragically short-sighted vision of the nation's future. I fervently urge our leadership - in the Congress and in the Administration - to prevent this from happening.

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[2] Science Indicators 1980 (Report of the National Science Board 1981).

[3] Willis H. Shapley, Albert H. Teich, and Jill P. Weinberg, Congressional Action on Research and Development in the FY 1982 Budget. American Association for the Advancement of Science Washington, D.C.) January, 1982.

HIGH ENERGY PHYSICS
TOTAL DOE FUNDING
(B/A IN FY 1983 DOLLARS)

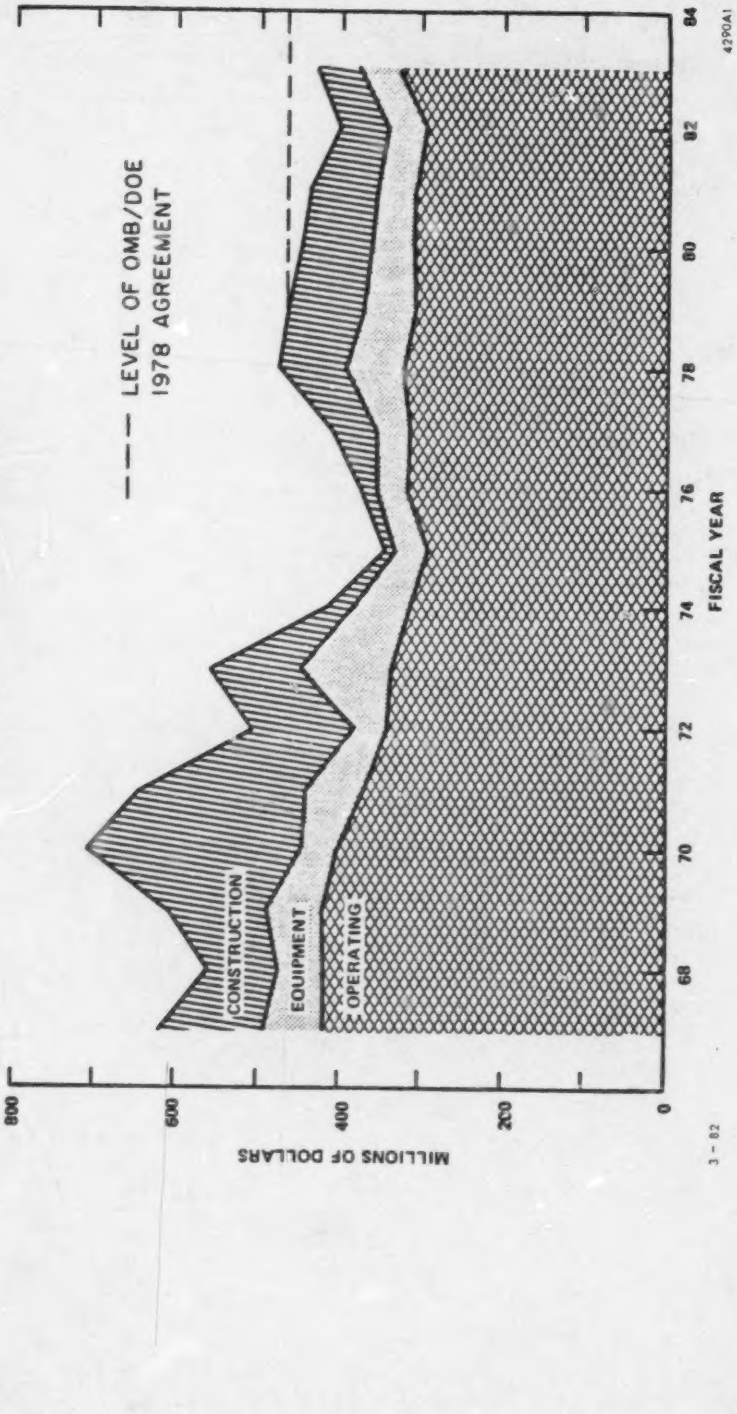
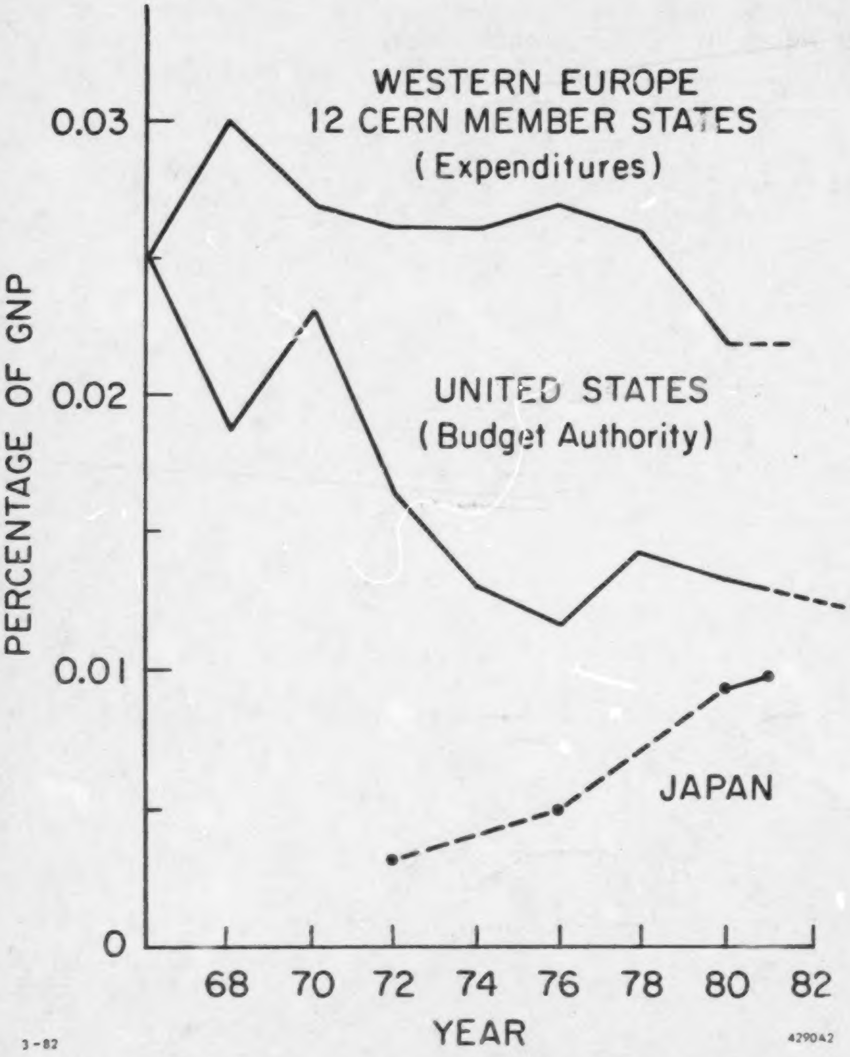


Fig. 1



Total cost of high energy physics as a percentage of GNP in Western Europe, Japan and the United States

Fig. 2

Mr. **Fuqua**. Thank you, very much, Dr. Drell.

We will now hear from Dr. Feshbach. Your full written testimony will appear in the record at this point.

[The biographical sketch of Dr. Feshbach follows:]

HERMAN FESHBACH
Curriculum Vitae

DATE OF BIRTH: February 2, 1917

EDUCATION:

College of the City of New York, S.B., 1937
Massachusetts Institute of Technology, Ph.D., 1942

TEACHING AND RESEARCH: Massachusetts Institute of Technology

1973-1982	Head, Department of Physics
1967-1973	Director, Center for Theoretical Physics
1955-	Professor of Physics
1947-1955	Associate Professor of Physics
1945-1947	Assistant Professor of Physics
1941-1945	Instructor

HONORS AND AWARDS:

1978-	Board of Governors, The Weizmann Institute of Science
1977	Townsend-Harris Medal, College of the City of New York
1976-	Cecil and Ida Green Professor of Physics
1976	Sc.D. (Honorary), Lowell Technological University
1975	de Shalit Memorial Lecturer The Weizmann Institute of Science
1974	Schiff Memorial Lecturer, Stanford University
1973	T.W. Bonner Prize in Nuclear Physics
1962	Ford Foundation Fellow
1954	Guggenheim Fellow
1943	Navy Ordinance Development Award

PROFESSIONAL SOCIETIES:

American Academy of Arts & Sciences, Fellow (1954); Vice President, Class I (1973-1976)
American Association for the Advancement of Science, 1969-, Fellow
Federation of American Scientists, National Council Member (1979-1983)
National Academy of Sciences, 1969-

STATEMENT OF DR. HERMAN FESHBACH, PROFESSOR OF PHYSICS AND HEAD OF THE DEPARTMENT, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, CAMBRIDGE, MASS., AND CHAIRMAN, DOE NSF NUCLEAR SCIENCE COMMITTEE

Dr. FESHBACH. Thank you, Mr. Chairman and members of the subcommittee. I am grateful for this opportunity to speak to you with regard to the program in the field of nuclear physics in the Department of Energy.

This is the fourth year that I am Chairman of the Nuclear Science Advisory Committee but, as important, I have been a nuclear physicist for some 40 years, so I can speak from some experience.

I did testify to this subcommittee a year ago and I will refrain from repeating in detail all of the things that were said then.

Obviously, I would like to emphasize the fundamental role played by the nucleus and nuclear processes. Briefly, our present universe is a nuclear one. Most of the mass of the universe is nuclear, most of the energy has had its origin in nuclear and gravitational processes. This has been the case from a time soon after the "Big Bang" to the present.

I shall not repeat the illustrations of the wide-ranging impact of basic nuclear science on national security, on industry, on energy production of both the fission and fusion variety, on medical diagnosis and therapy, on the other sciences such as astrophysics, material sciences, archeology, and biology. To these should be added the contributions to technology which are made in the course of construction of accelerators and detectors. The modern computer is one example of such a spinoff; superconducting technology is another.

My emphasis this year will be on what we believe to be the important scientific opportunities which present themselves at the present time. These research directions are important, not only because they will uncover properties of nuclei and nuclear processes, but also because they will reveal phenomena and concepts of great generality, which will be of significance for all science.

Let me say at this point that I believe we are on the threshold of an unusually strong advance in our understanding of nuclear phenomena. Of course, we shall be able to cross that threshold only if our environment is supportive. I hope this subcommittee will see fit to authorize the means required to make this prospect a reality.

Two of the great discoveries made in nuclear science revealed the existence of the strong and the weak forces. Nuclear forces, those which exist between the nuclear particles, are the strongest known to nature. The weak forces, on the other hand, are responsible for a form of radioactivity. These forces, together with the electromagnetic and gravitational forces, are the fundamental forces which govern all the phenomena of the physical universe, whether it be the structure of large molecules or the energy production in the stars or the production of chemical elements during the course of stellar evolution.

The next decade promises a significant progress in our understanding of the strong forces. This has two complementary aspects. One is the direct study of the forces acting between quarks, the constituents of the neutrons and protons making up the nucleus, responsible for the forces between neutrons and protons.

It turns out that that part of these quark-quark forces, which is poorly understood, that part which leads to containment, can be studied in nuclei through the use of appropriate probes. One of these involves high energy electrons striking nuclei. This is the preferred mode of attack on this problem. But the collision of high energy protons and high energy nuclei with nuclei, also shows a great deal of promise in this regard. In the last case, and this is highly speculative, it is possible that a new form of matter, quark matter, might be formed.

On the other hand, we can also study how the strong forces bind the neutrons and protons to form the nucleus. The most significant discovery, a discovery of great generality and importance made by nuclear physicists in this area, is the existence of collective motions produced by an underlying dynamic mean field of force. The word "collective" refers to the fact that a substantial fraction of the nucleons forming the nucleus are moving together in an organized way. These collective motions are the elementary particles of nuclear physics. Much of the dynamic behavior of nuclei can be understood in terms of these "elementary particles" and their interaction. This is a remarkable result, totally unexpected and originally very mysterious.

Two Nobel Awards were made for the discovery of two such collective motions. An obvious question arises: Are there others? The answer is that there are and several have been uncovered. Importantly for the future as we use different beams produced by our accelerators, and more ingenious ways of looking at the consequences, other will be found.

In the last 2 years three such "elementary particles" have been found using electron, proton, and heavy ion beams. I could go on to give other examples of suspected "elementary particles" not yet proved and I am certain there are some that are not even glints in the eyes of the nuclear physicist. Suffice it to say this is a very rich field, full of surprises and opportunities.

Let me turn next to the questions associated mostly with the collision of nuclei, referred to as heavy ion research in the budget. A very wide range of phenomena have been revealed. At very high energies the colliding nuclei, one or both, will literally explode into its constituent nucleons. At lower energies, the colliding nuclei come into intimate contact exchanging energy and matter, forming new nuclei in various states of motion. The fundamental questions to be answered in both examples and in those which lie in between are how are energy and matter transferred for different experimental regimes, how are these best described, and is any kind of equilibrium established when the nuclei are in intimate contact? Moreover, in these collisions the nuclei will undergo extraordinarily extreme types of motion and stress. A study of their response will reveal properties of the strong forces which can be gained in no other way.

We turn next to the weak force responsible for radioactivity. It was a nuclear experimentalist C. S. Wu who verified the brilliant suggestion made by T. D. Lee and C. N. Yang regarding the nature of the weak forces. More recently the weak and electromagnetic forces have been united in the so-called gage theory which Dr. Drell referred to. The present day "standard theory" may not yet be the final formulation of this unification. There are many aspects which need to be tested. It is again the nucleus, using polarized electron beams, which will provide the most important tests. These experiments are being planned, equipment is being built, and we can confidently expect some crucial experiments to be done in the near future.

In another set of remarkable experiments the weak component of forces which exist between nucleons with a strength of 1/10,000,000 of the strong forces has been detected opening up a new set of investigations of the nature of the weak forces.

Nuclear science is a most important area of modern physics. It affords the most effective way of studying the strong interactions as well as providing important ways of studying the weak interactions. The properties of these forces are the focus of attention of much of modern physics.

Let me now turn away from science and go on to budgetary considerations.

In view of these very bright prospects for making contributions of a truly fundamental nature, one must regard the fiscal year 1983 in which the expenditures are kept constant, in current dollars at about $130 million—effectively a substantial reduction when inflation is taken into account—as unfortunate. The consequences are truly dire. The U.S. capability to do research in nuclear physics will be substantially reduced. An important accelerator, at the present time the best of its kind, operating most effectively will have to be shut down. Another major accelerator will be underutilized, and still another will be unable to carry out associated medical applications.

Two university accelerators are being terminated, while the few remaining suffer because of the obsolescence of their equipment. This last decade, the number of university accelerators have been reduced by a factor of 10; that is, one-tenth of the universities that had accelerators at the beginning of the 1970's have them now. Accompanying this reduction will be the eventual dismissal of highly trained personnel and the dissolution of research teams which it took many years to form.

To be more specific, let me turn to the actual DOE budget and, if I may, Mr. Chairman, use the projector.

To be more specific, let us turn to the DOE fiscal year 1983 congressional budget request. Page 49 gives the fiscal year 1983 request for nuclear physics. We see that the fiscal year 1983 total to which $6.8 million should be added is roughly $3.5 million more than the fiscal year 1982 appropriation.

Let me point especially to the fact that operating expenses have increased by only $1 million. The consequences are indicated below. We see that medium energy nuclear physics and accelerator operations will be 5 percent below 1982 levels of activity. We see that the superb accelerator at Brookhaven will begin to be phased out as well as the accelerators at the University of Colorado and the University of Wisconsin. This leaves, in fact, only five university-based accelerators funded by DOE. Of the ones that remain, the University of Washington and Yale are in urgent need of construction funds. The table does not reveal the reduced capability of the remainder of nuclear physics institutions to perform front line research.

It is useful to look at the history of funding in the field of nuclear sciences and see how this looks over the last few years.

First of all, here is the funding of the U.S. programs in 1972 dollars with the fiscal year at the bottom and millions of dollars at the top. If you look at the deflated dollars, you see a very sharp reduction. If I go on and you look at that particular piece more directly, here it is in 1977 dollars, and you see that we have gone down from some 20 percent in the 1983 budget.

Although this is not exactly the problem of this committee, I cannot help but put in this NSF funding which shows an enormous drop of some 25 percent from the past.

During last year's testimony I referred to the long-range plan, developed by the Nuclear Science Advisory Committee in 1979, I would like to remind you that that long-range plan discussed in some detail in last years testimony, which was worked out in 1979 with the assistance of the Department of Energy and the National Science Foundation. It has been accepted by, the DOE, the NSF, and the OSTP as a framework for the management of the nuclear physics program supported by the NSF and the DOE. It is useful at this point to refer to the deviation of the projections of that plan from what has actually occurred. I might point out that that plan is a truly conservative one and is an attempt to keep constant the scientific effectiveness of the U.S. nuclear program.

In the next transparency one sees the growing duration of the operating funds from that projected by the plan. The construction budget has been running roughly at the rate of $8 million per year which is less than one-half of what we thought it should be.

Earlier this week I chaired a meeting of the Nuclear Science Advisory Committee in which facilities proposals were made. I can tell you that they were all, without exception, excellent proposals attempting to probe frontier areas in an important way. I do not see how we can maintain that kind of forward-looking effort at the present budget level. Without beams of protons, without beams of electrons, without beams of neutrons, without pions, without heavy ions, without kaons, there is no way that the United States can be a part of the investigations of significant frontier issues or take advantage of the scientific opportunities as they present themselves.

I will say it this way. Not only is the present shortchanged but we are also compromising the future very seriously. The only renewable resource, and the most important renewable resource, is people. A way to look at that, I think, is to look at the Ph. D. production in the field of nuclear physics. This is 1980 [pointing] and this is 1960. The peak, then, is around 1971 or 1972. The total there is 240. In 1980 the number of Ph. D.'s awarded in nuclear physics was 73, of which 57 were United States citizens. I might point out that this number is much less than that which prevailed even in the pre-Sputnik era. We really have a terrible concern with respect to the future of the field. It cannot survive without young people entering it.

Finally, let me mention what our foreign colleagues are doing. In last year's testimony I compared the expenditures made by West

Germany and Switzerland in the field of nuclear science. To summarize that discussion, we note that about four times as big a fraction of the gross national product is spent on basic research in nuclear physics by these countries as is spent in the United States. Stated another way, the research funding per nuclear physicist in these countries is about twice that which is provided a U.S. nuclear physicist. The impact of this difference is already visible in the sharply increasing ratio of non-U.S. to U.S. publications in this field. To these remarks I should add that France, Japan, and Canada are embarked on a program of greatly strengthening their science programs. In France the funding will go up by 50 percent; similarly in Japan and Canada. Our northern neighbor, the 1981-82 budget is some 16 percent larger than the 1980-81 budget after correction for inflation; this is for science.

Mr. Chairman and members of the subcommittee, in view of these circumstances, in view of the importance of the field of nuclear physics for science, for our national security, for our economy, for our energy production technology, and for our health services, I would recommend to you the increase in the budget for the nuclear physics program for fiscal year 1983 by $15 million to approximately $145 million, corresponding to a thirteen percent increase over fiscal year 1982. It would be a profound important signal to our community and to our students and importantly it would enable the United States to be at the frontiers of nuclear science.

Let me conclude by thanking you once again for the opportunity to discuss these matters with you.

[The prepared statement of Dr. Feshbach follows:]

STATEMENT OF DR. HERMAN FESHBACH, CECIL AND IDA GREEN PROFESSOR OF PHYSICS, MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Mr. Chairman and Members of this Subcommittee, my name is Herman Feshbach. I am the Head of the Department of Physics at the Massachusetts Institute of Technology. I have served as President of The American Physical Society. I am Chairman of the DOE/NSF Nuclear Science Advisory Committee (NSAC), a post in which I am now serving for a fourth year. Previously I was Chairman of the NSAC Subcommittee on Facilities. I have been a nuclear physicist for over forty years. I am grateful for this opportunity to speak with you regarding the Program in the field of Nuclear Physics in The Department of Energy. I wish to stress that I am speaking to you as an individual and not in behalf of M.I.T. or any other organization.

It is appropriate to recall that I testified to this Subcommittee one year ago, March 10, 1981, with regard to the fiscal year 1982 nuclear physics program of the Department of Energy.

SCIENTIFIC BACKGROUND

I shall not repeat last year's discussion of the fundamental role played by the nucleus and nuclear processes. Briefly our present universe is a nuclear one. Most of the mass of the universe is nuclear, most of the energy has had its origin in nuclear and gravitational processes. This has been the case from a time soon after the big bang to the present. I shall not repeat the illustrations of the wide ranging impact of basic nuclear science on national security, on industry, on energy production of both the fusion and fission variety, on medical diagnosis and therapy, on other sciences such as astrophysics, material science, archeology and biology. To these should be added the contributions to technology which are made in the course of construction of accelerators and detectors. The modern computer is one example of such a spin-off; superconducting technology is another.

My emphasis this year will be on what we believe to be the important scientific opportunities which present themselves at the present time. These research directions are important, not only because they will uncover properties of nuclei and nuclear processes, but also because they will reveal phenomena and concepts of great generality, which will be of significance for all science. I shall focus on this aspect rather than provide you with a catalogue of investigations to be carried out in the near and far future.

Let me say at this point that I believe that we are on the threshold of an unusually strong advance in our understanding of nuclear phenomena. Of course, we shall be able to cross that threshold only if our environment is supportive. I hope this Subcommittee will see fit to authorize the means required to make this prospect a reality.

Two of the great discoveries made in nuclear science revealed the existence of the strong and the weak forces. Nuclear forces, those which exist between the nuclear particles, neutrons and protons which make up the nucleus, are the strongest known to nature. They hold the nucleus together which because of the strength of these forces forms a very dense system. The weak forces are, on the other hand, responsible for a form of radioactivity. These forces together with the electromagnetic and gravitational forces are the fundamental forces which govern all the phenomena of the physical universe, whether it be the structure of large molecules or the energy production in the stars or the production of the chemical elements during the course of stellar evolution.

The next decade promises significant progress in our understanding of the strong forces. This has two complementary aspects. One is the direct study of the forces acting between quarks, the constituents of the neutrons and protons making up the nucleus, responsible for the forces between the neutrons and protons. It turns out that that part of these quark-quark forces which is poorly understood, that part which leads to containment, can be studied in nuclei through the use of appropriate probes. One of these involves high energy electrons striking nuclei. This is the preferred mode of attack on this problem. But the collision of high energy protons and high energy nuclei with nuclei also shows a great deal of promise in this regard. In the last case, and this is highly speculative, it is possible that a new form of matter, quark matter, might be formed.

On the other hand, we can also study how the strong forces bind the neutrons and protons to form the nucleus and learn thereby how the nuclear environment affects nuclear forces. We have a good opportunity to study many aspects of this problem because there are many nuclei. It is also the reason why we find the production of new elements and new forms of elements found naturally so important. The most significant discovery, a discovery of great generality and importance, made by nuclear physicists in this area, is the existence of collective motions produced by an underlying dynamic mean field of force. The word "collective" refers to the fact that a substantial fraction of the nucleons forming the nucleus are moving together in an organized way. These collective motions are the elementary particles of nuclear physics. Much of the dynamic behavior of nuclei can be understood in terms of these "elementary particles" and their interaction. This is a remarkable result totally unexpected and originally very mysterious. Two Nobel Awards were made for the discovery of two such collective motions. An obvious question arises: Are there others? There are. Several have been uncovered. Importantly for the future as we use different beams produced by our accelerators, and more ingenious ways of looking at the consequences, others will be found. In the last two years three such "elementary particles" have been found using electron, proton and heavy ion beams. Another, the formation of nuclear molecules, two nuclei revolving about each, seems to be a much more general phenomenon than we at first thought. I could go on to give other examples of suspected "elementary particles", and I am certain there are some that are not even glints in the eyes of the nuclear physicist. Suffice it to say this is a very rich field, full of surprises and opportunities.

Let me turn next to the questions associated mostly with the collision of nuclei, referred to as heavy ion research in the budget. A very wide range of phenomena have been revealed. At very high energies the colliding nuclei, one or both, will literally explode into its constituent nucleons. At lower energies, the colliding nuclei come into intimate contact exchanging energy and matter, forming new nuclei in various states of motion. The collision is in this case rather peaceful. The fundamental questions to be answered in both examples and in those which lie in between are how are energy and matter are transferred for different experimental regimes, how are these best described, and is any kind of equilibrium established when the nuclei are in intimate contact. Moreover, in these collisions the nuclei will undergo extraordinarily extreme types of motion and stress. A study of their response will reveal properties of the strong forces which can be gained in no other way.

In summary, the study of nuclei and nuclear collisions are uniquely the most effective way of studying the strong forces, one of the four fundamental forces which govern the dynamics of the physical universe.

We turn next to the weak force responsible for radioactivity. It was a nuclear experimentalist who verified the brilliant suggestion made by T. D. Lee and C. N. Yang regarding the nature of the weak forces. More recently the weak and electromagnetic forces have been united in the so-called gauge theory. The present day "standard theory" may not yet be the final formulation of this unification. There are many aspects which need to be tested. It is again the nucleus, using polarized electron beams, which will provide the most important tests. These experiments are being planned, equipment built; and we can confidently expect some crucial experiments to be done in the near future. In another set of remarkable experiments the weak component of forces which exist between nucleons with a strength 1/10,000,000 of the strong forces has been detected opening up a new set of investigations of the nature of the weak forces.

Nuclear science is a most important area of modern physics. It affords the most effective way of studying the strong interactions as well as providing important ways of studying the weak interactions. The properties of these forces are the focus of attention of much of modern physics.

BUDGETARY CONSIDERATIONS

In view of these very bright prospects for making contributions of a truly fundamental nature, one must regard the proposed fiscal year 1983 budget in which the expenditures are kept constant, in current dollars at about $130 M (effectively a substantial reduction when inflation is taken into account) as unfortunate. The consequences are truly dire. The U.S. capability to do research in nuclear physics will be substantially reduced. An important accelerator, at the present time the best of its kind, operating most effectively will have to be shut down as mentioned in the budget description. Another major accelerator will be underutilized, and still another will not be able to carry out associated medical applications. Two university accelerators are being terminated while the few remaining suffer because of the obsolescence of their equipment. This last decade the number of university accelerators have been reduced by a factor of ten; that is, one-tenth of the universities that had accelerators at the beginning of the 70's have them now. Accompanying this reduction will be the eventual dismissal of highly trained personnel and the dissolution of research teams which it took many years to form.

To be more specific, let us turn to the DOE fiscal year 1983 Congressional Budget Request. Page 49 gives the fiscal year 1983 request for nuclear physics. We see that the fiscal year 1983 total to which $6.8 M should be added is roughly $3.5 M more than the fiscal year 1982 appropriation. Let me point especially to the fact that operating expenses have increased by only $1 M. The consequences are indicated below. We see that medium energy nuclear physics and accelerator operations will be 5 percent below 1982 levels of activity. We see that the superb accelerator at Brookhaven will begin to be phased out as well as the accelerators at the University of Colorado and the University of Wisconsin. This leaves, in fact, only five university-based accelerators funded by DOE. Of the ones that remain, the University of Washington and Yale are in urgent need of construction funds. The table does not reveal the reduced capability of the remainder of nuclear physics institutions to perform front line research.

It is useful to look at the history of funding in the field of nuclear science. The first transparency gives the history going back to 1960. The situation is given in more detail in the next transparency which shows a decrease of more than 20 percent since 1977. The third shows the sharp decline in NSF funding, about 25 percent since 1977. The impact of these declines is exacerbated by the fact that in this time period the new accelerators which have been built are more expensive to operate. It is also useful to compare the history of funding and the funding which was projected by the DOE/NSF Nuclear Science Advisory Committee in its Long Range Plan for Nuclear Science (December 1979), a plan developed in cooperation with DOE and NSF. That plan has been accepted by the DOE, the NSF, and the OSTP as a framework for management of the Nuclear Physics Program supported by the DOE and NSF in basic nuclear research. The plan was discussed in last year's testimony. It should be emphasized that it is a conservative one designed to keep a constant level of scientific strength. In view of the erosion of the support for nuclear physics I have just documented, it is not at all surprising that the goals of this plan have not been met. This is illustrated in the next transparency which shows the growing discrepancy in operating funds. In the case of line item construction the appropriations since fiscal year 1978 (including the funds for MSU) have averaged about $8M per year. This is less than one-half of the amount which the plan proposed in order to maintain constant scientific strength. Construction of accelerators which will provide beams of electrons, protons, neutrons, pious heavy ions, kaons are essential to this field. Without these facilities, the U.S. nuclear program will no longer be able to investigate the significant frontier issues or take advantage of the scientific opportunities as they present themselves.

Not only is the present shortchanged, but the future of nuclear physics is compromised. That future is critically dependent upon the flow of young people into the field. That flow has been drying up at an alarming rate as the last transparency demonstrates. We see that the number of Ph.D.'s awarded in the field in 1980 is 73, 32 percent of the peak. Of these 73, only 57 are U.S. citizens. We note that the number is much less than that which prevailed even in the pre-Sputnik era. This decline is a reflection of the students' perception that in view of the eroding support, the opportunities for a productive and challenging career are small. The prospect of engaging in arduous, lengthy, albeit fascinating course of study in face of this perception is hardly attractive. Shortages of manpower are already apparent. In addition, as can be readily documented, the training a nuclear physicist receives is an excellent and very broad one. Over the years many have gone into applied nuclear physics, industry, and medicine and have proven to be extraordinarily effective. In my opinion this decrease in the number of young people going into nuclear physics and into the physical science forms a window of vulnerability which is of great danger to our society.

In last year's testimony I compared the expenditures made by West Germany and Switzerland in the field of nuclear science. To summarize that discussion, we note that about four times as big a fraction of the gross national product is spent on basic research in nuclear physics by these countries as is spent in the United States. Stated in another way, the research funding per nuclear physicist in these countries is about twice that which is provided a U.S. nuclear physicist. The impact of this difference is already visible in the sharply increasing ratio of non-U.S. to U.S. publications in this field. To these remarks I should add that France, Japan and Canada are embarked on a program of greatly strengthening their science program. In France the funding will go from 1.8 percent to 2.5 percent of the GNP. In Japan the rise will be from 2 percent to 3 percent. In Canada the 1981-1982 budget in some 16 percent larger than the 1980-1981 budget after correction for inflation.

Mr. Chairman and Members of the Subcommittee, in view of these circumstances, in view of the importance of the field of nuclear physics for Science, for our national security, for our economy, for energy production technology, for our health services, I would recommend increasing the budget for the Nuclear Physics Program by $15 M to approximately $145 M, corresponding to about a 13 percent increase over fiscal year 1982. It would be a profound important signal to our community and to our students and importantly it would enable the U.S. to be at the frontiers of nuclear science.

Let me conclude by thanking you once again for the opportunity to discuss these matters with you.

JAN 19 1982

Basic Sciences
General Science and Research - Operating Expenses
General Science and Research - Plant and Capital Equipment

(Tabular dollars in thousands. Narrative material in whole dollars.)

	FY 1981 Appropriation	FY 1982 Appropriation	FY 1983 Base	FY 1983 Request
Nuclear Physics				
Medium energy nuclear physics				
Operating expenses	$ 46,000	$ 50,600	$ 50,600	$ 53,100
Subtotal	46,000	50,600	50,600	53,100
Heavy ion nuclear physics				
Operating expenses[1]	39,060	41,814	41,814	39,700
Subtotal	39,060	41,814	41,814	39,700
Low energy nuclear physics				
Operating expenses[2]	11,582	12,257	12,257	12,400
Subtotal	11,582	12,257	12,257	12,400
Nuclear theory				
Operating expenses	7,000	7,700	7,700	8,350
Subtotal	7,000	7,700	7,700	8,350
Capital equipment[3]	9,870	9,510	9,510	9,900
Construction	11,300	12,000	12,000	7,700
Total				
Operating expenses	103,642	112,371	112,371	11[illegible],350
Capital equipment	9,870	9,510	9,510	9,900
Construction	11,300	12,000	12,000	7,700[4]
Nuclear Physics	$124,812	$133,881	$133,881	$130,950

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[1] Includes $2,560,000 in FY 1981 and $2,714,000 in FY 1982 which was budgeted under Life Sciences and Nuclear Medicine Applications for Bevalac facility operations. In FY 1983, Bevalac facility operations for all research, including biomedical, is budgeted entirely under Nuclear Physics.

[2] Budgeted under Nuclear Science subprogram of Basic Energy Sciences in FY 1981 and FY 1982.

[3] Includes $470,000 in FY 1981 and $510,000 in FY 1982 which was budgeted under Nuclear Science subprogram of Basic Energy Sciences.

[4] Reflects transfer of the National Superconducting Cyclotron Laboratory construction project (80-GS-5) to National Science Foundation; prior to transfer $6,800,000 was planned for this project in FY 1983.

AUTHORIZATION: Sec. 209, P.L. 95-91

Summary of Changes

FY 1982 Appropriation enacted	$133,881
Built-in increases and decreases	0
FY 1983 Base	$133
Program Increases and Decreases	
Nuclear Physics	
Conduct medium energy nuclear physics research and accelerator operations at about 5% below FY 1982 levels of activity	+ 2,
Initiate phase out of Brookhaven Double MP research and accelerator operations and conduct remaining heavy ion nuclear research and accelerator operations at about 10% below FY 1982 levels of activity	- 2,

Phase out research and accelerator operations at the University of Wisconsin Tandem Van de Graaff and University of Colorado Cyclotron facilities and conduct remaining low energy nuclear research and accelerator operations at about FY 1982 levels of activity	+ 143
Conduct nuclear theory research at about FY 1982 levels of activity..	+ 450
Complete funding for Argonne Tandem/Linac Accelerator System (ATLAS) so that construction project can remain on schedule and provide beams needed for heavy ion nuclear research	- 300
Transfer National Superconducting Cyclotron Laboratory (Michigan State University) construction project to National Science Foundation	- 4,500
Provide capital equipment necessary to conduct research programs including long lead time items for ATLAS experimental areas	+ 390
Modernize existing facilities to meet demands of frontier nuclear research by provision of Accelerator Improvement Project and General Plant Project funds	+ 500
FY 1983 Budget Request	$130,950

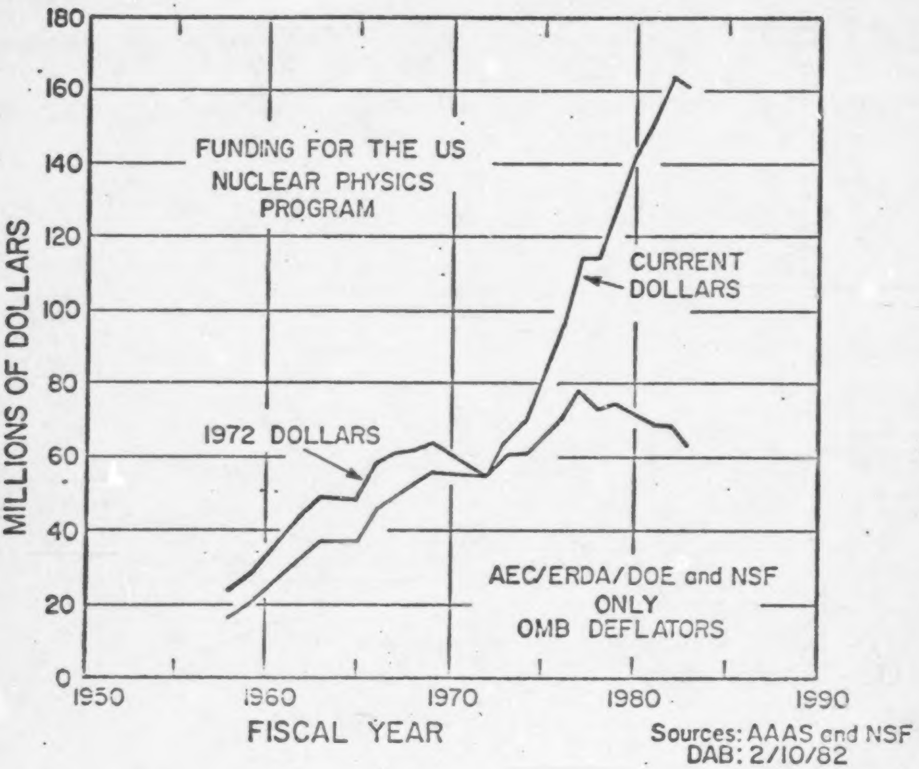
Introduction

Nuclear matter in the form of atomic nuclei and massive astrophysical formations such as neutron stars is believed to be the most common substance in the universe but is, at the same time, held together by the most powerful forces of nature. The long range goals of the Nuclear Physics program continue to be an understanding of the interactions, properties, and structures of atomic nuclei and nuclear matter at the most fundamental level possible and an understanding of the fundamental forces of nature by using nuclei as a proving ground. Ultimate realization of these goals would be quantitative description of all of the great diversity of structures, properties, and interactions of nuclei in terms of the most fundamental forces and components of our universe. As with many human goals, ultimate realization appears to be beyond our immediate grasp. However, pursuit of these goals yields an ever deeper understanding of nuclei, their role in the physical universe about us, and also a national base of knowledge, techniques, instrumentation, and skilled manpower in critical technologies.

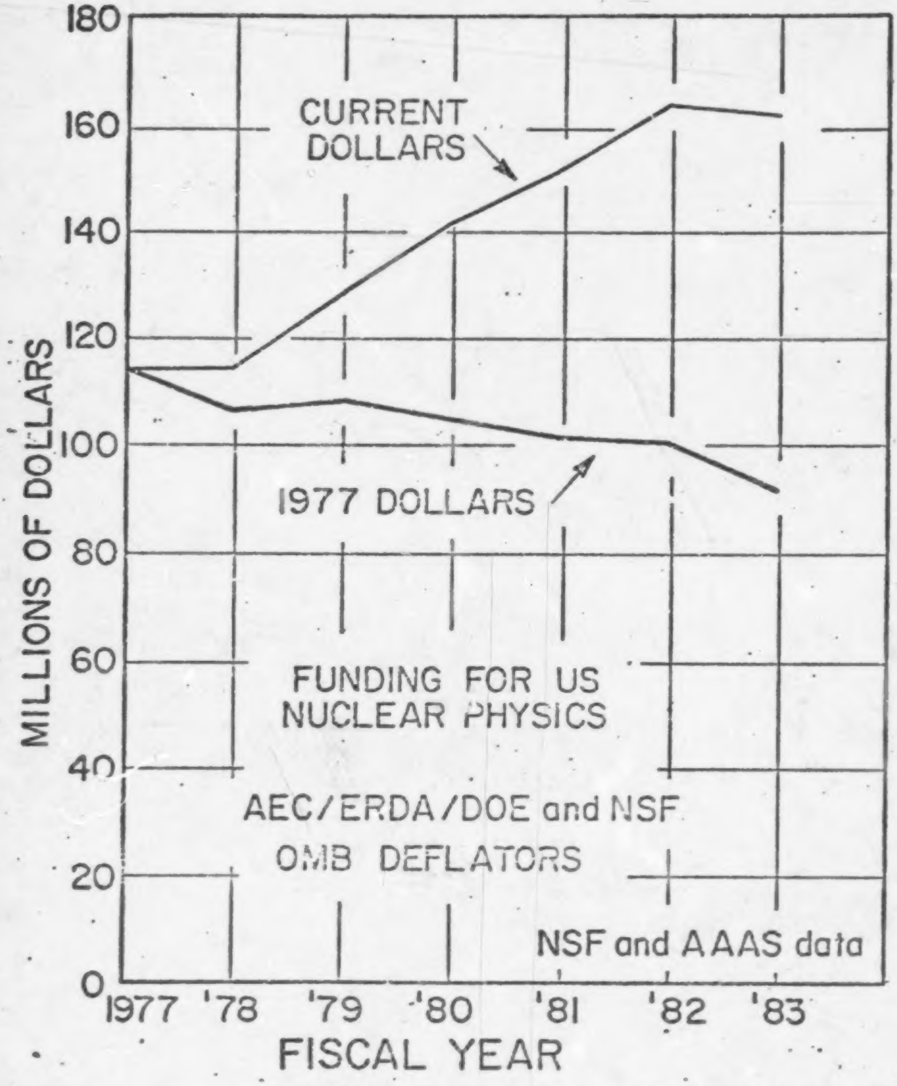
Nuclear Physics is a comprehensive program of interdependent experimental and theoretical investigations of atomic nuclei. Basic ingredients of the program must include talented and imaginative scientists and a diversity of facilities to provide the variety of probes, instruments, and computational equipment needed for modern nuclear research. Over 80% of the total Federal support of basic nuclear research is provided through the Nuclear Physics program. This agency functions as the Federal executive agent for this essential area of basic science.

Long Range Planning

In cooperation with this agency and the National Science Foundation (NSF), [illegible]/NSF Nuclear Science Advisory Committee (NSAC) has developed a long range plan [illegible]ic nuclear research -- "A Long Range Plan for Nuclear Science" (December 1979) [illegible] Plan has been accepted by this agency, National Science Foundation, and the Office [illegible]ence and Technology Policy as a framework for management of Nuclear Physics Program[illegible]orted and National Science Foundation-supported work in basic nuclear research. Ob[illegible]ve of the Plan is a coordinated Federal program of basic nuclear research. The Nuclear Physics budget request for FY 1983 is formulated within the scientific guidelines set forth by the NSAC Long Range Plan. However, the request falls short of the budgetary projections of the Plan.



Sources: AAAS and NSF
DAB: 2/10/82


180
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MILLIONS OF DOLLARS
CURRENT DOLLARS
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FUNDING FOR US NUCLEAR PHYSICS
AEC/ERDA/DOE and NSF
OMB DEFLATORS
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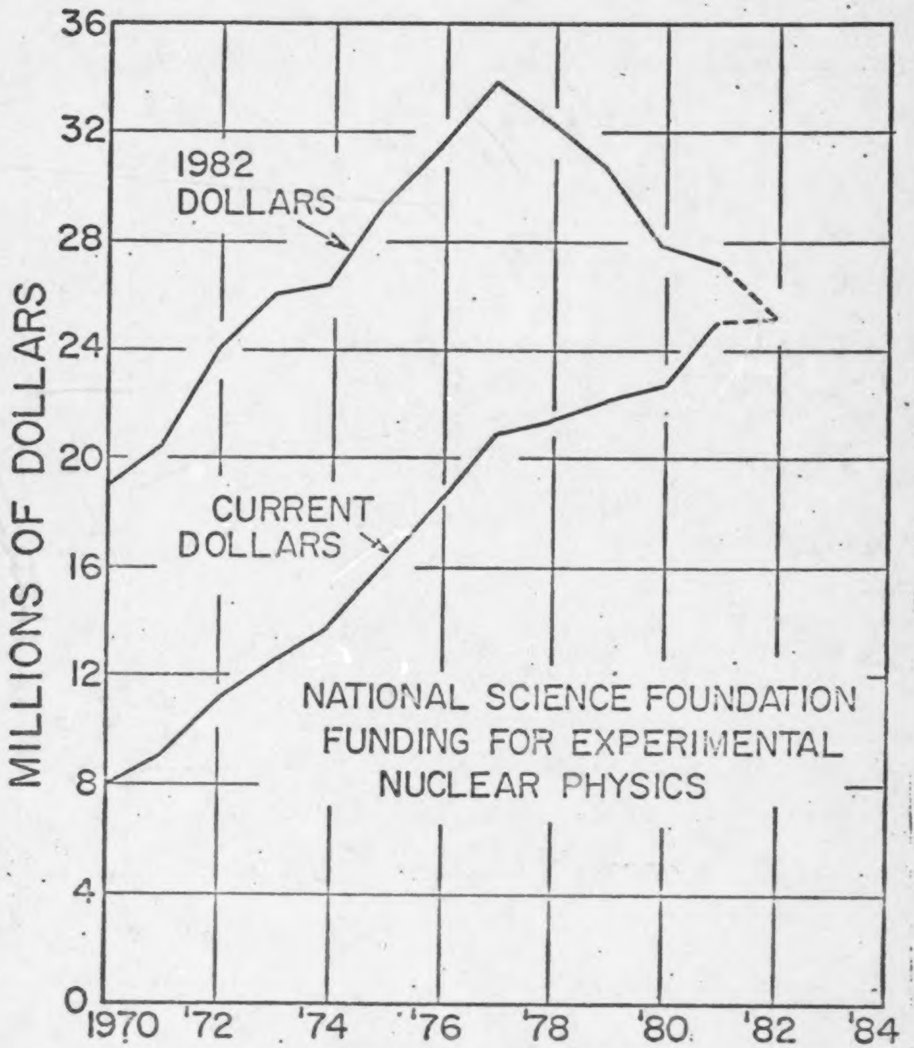
MILLIONS OF DOLLARS
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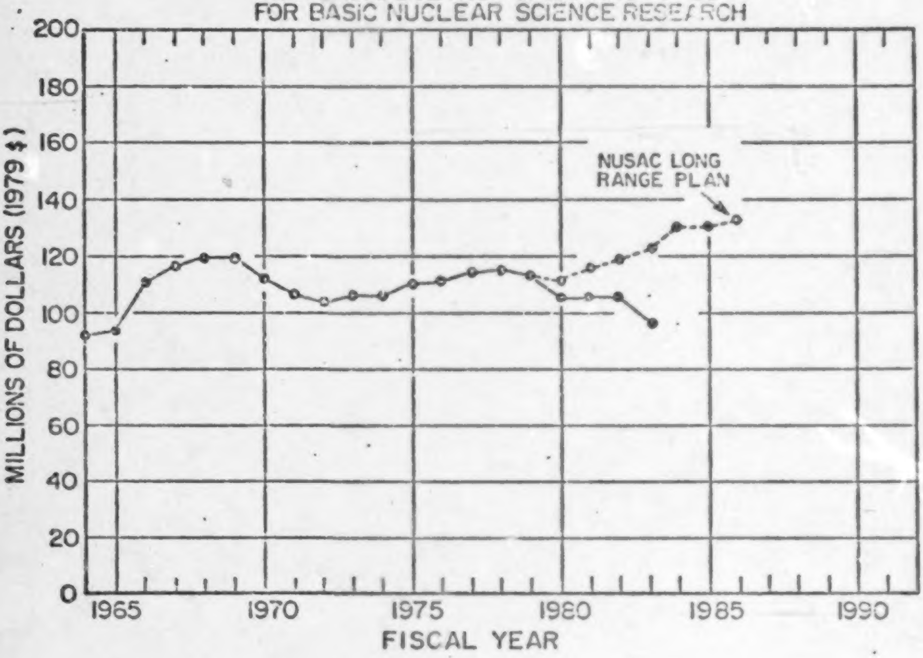
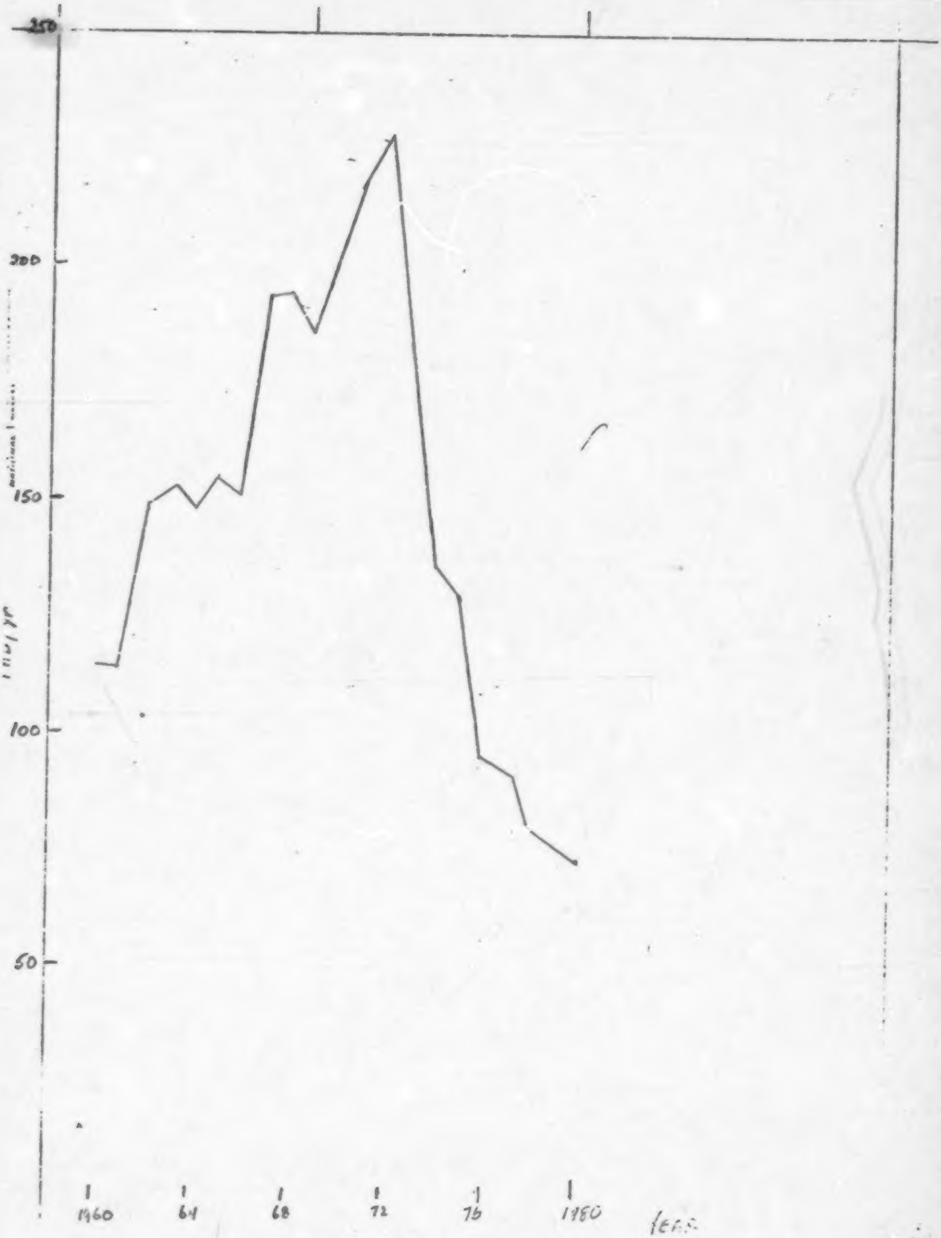
NATIONAL SCIENCE FOUNDATION FUNDING FOR EXPERIMENTAL NUCLEAR PHYSICS



AEC/ERDA/DOE+NSF OPERATING FUNDS FOR BASIC NUCLEAR SCIENCE RESEARCH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YEAR

Mr. **Fuqua**. Thank you, very much, Dr. Feshbach. I would like to call on our colleague from Tennessee, Mrs. Bouquard who chairs the Subcommittee on Energy Research and Production. I gather she has some personal knowledge of the next witness and where he is associated.

Mrs. **Bouquard**. I certainly do, Mr. Chairman.

Thank you, very much, Mr. Chairman. It is always good to see my constituents from Oak Ridge here testifying before our committee and it certainly gives me a great pleasure this morning to introduce Dr. Alex Zucker. He is a friend of mine and he is also associate director for physical sciences of the Oak Ridge National Laboratory and a distinguished nuclear physicist in his own right.

Dr. Zucker has published numerous technical papers, including invited review articles in a variety of prestigious journals throughout his career since he received a Ph. D. from Yale in 1950.

He came to ORNL over 30 years ago and has performed in a manner which makes me very comfortable using the word "excellent." Dr. Zucker has served on ma[illegible] ranging from those of the National Academy of Sciences [illegible] variety of important international committees. He has an [illegible]ational reputation as a researcher and a scientist who possesses a broad vision of where our U.S. efforts are going and our problems and opportunities in basic research.

Today, he will be giving testimony on the DOE program in basic energy sciences. These BES activities have received strong support from the committee and I believe on the basis of Dr. Zucker's testimony, we can feel very certain that these funds are well spent.

Alex, I am delighted to welcome you here this morning. I only regret that my subcommittee is also meeting at the same time.

Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, very much, Mrs. Bouquard.

Dr. Zucker, we are pleased to hear from you at this time and your full written testimony will appear in the record.

[The biographical sketch of Dr. Zucker follows:]

ALEXANDER ZUCKER

Address:

Oak Ridge National Laboratory
P. O. Box X, Oak Ridge, Tennessee 37830

103 Orange Lane, Oak Ridge, Tennessee 37830

Education:

B. A., University of Vermont, 1947.
M. S., Yale University, 1948.
Ph.D., Yale University, 1950.

Occupation:

1973-present	Associate Director for Physical Sciences, Oak Ridge National Laboratory
1972-1974	Director, Heavy-Ion Project, ORNL
1970-1972	Executive Director, Environmental Studies Board, National Academy of Sciences/ National Academy of Engineering
1968-1972	Ford Professor of Physics, University of Tennessee
1960-1970	Associate Director, Electronuclear Division, ORNL
1950-1960	Physicist, ORNL

Honors:

1981-1982	Editorial Board of Science.
1980-	Editor of Nuclear Science Applications, Section B.
1979	Member of National Academy of Sciences Nuclear Physics Delegation to the People's Republic of China.
1978-	Member of Research Coordination Panel of the Gas Research Institute.
1976-1978	Member-at-large, U. S. National Committee for the International Union of Pure and Applied Physics.
1971	Delegate to Pugwash Conference.
1966-1967	John Simon Guggenheim Fellow and Fulbright-Hays Research Scholar, Rutherford Laboratory, England.
1963	Member of U. S. delegation to the U.S.S.R. on peaceful uses of atomic energy.
	Member of NAS/NRC Committee on Nuclear Science.
	Member of Nuclear Physics Panel, NAS/NRC Physics Survey.
	Fellow, American Physical Society.
	Fellow, American Association for the Advancement of Science.
	Phi Beta Kappa.
	Sigma Xi.

Fields of Research:

Nuclear Physics.
Nuclear reactions induced by medium energy protons (30 to 60 MeV); polarization phenomena with 40-MeV protons; few-nucleon interactions, for example deuterons on ^{3}He; total reaction cross sections with protons; heavy-ion transfer reactions; scattering of nitrogen ions from light nuclei; angular momentum effects in heavy-ion collisions; passage of energetic heavy ions through matter; equilibrium charge distribution; pickup and loss of electrons. Ion sources; accelerators, especially cyclotrons.

STATEMENT OF DR. ALEXANDER ZUCKER, ASSOCIATE DIRECTOR, OAK RIDGE NATIONAL LABORATORY, OAK RIDGE, TENN.

Dr. ZUCKER. Thank you, Mr. Chairman. Thank you, Mrs. Bouquard, for your fine words.

It is a great pleasure and honor to be here today and in the interests of time I will simply cover the highlights and most important facts and conclusions of my testimony which I have submitted.

I will respond to three points in your invitation to testify. First, the quality of the BES research; second, the relevance of the BES research to strengthening the energy technology base; and third, the adequacy of the level of Federal support.

First, on the quality of the research. In 1981 the Office of Program Analysis of the Office of Energy Research carried out a major review of the basic energy sciences program; 129 projects out of a total of 1,200 were reviewed in depth. To insure fairness, the projects were selected randomly, the reviewers were drawn from the scientific community with a view to their expertise and capability to review the projects; 83 of the reviewers came from universities, 34 from industry, 38 from the national laboratories, and 25 from other institutions.

Two sets of criteria were given to the reviewers: One dealing with the quality of research, and the other one dealing with the relevance of that research to the energy mission. Each project was rated on a scale from 0 to 10 and in the first slide I show the definitions of what those grades are.

As you can see, an outstanding project was graded 9 or 10; a strong project deserving priority support, 7 or 8; a project with negligible shortcoming, 5 or 6; and projects that require intervention by the basic energy sciences' staff were rated 3 and 4, and 0 to 2.

The next viewgraph shows the results and it is most gratifying to be able to say today that 91 percent of all the projects reviewed had a grade of five or better. In other words, there were no important shortcomings in any of them.

About 60 percent are deserving of priority support or are outstanding projects. The 12 projects or so that fall below the line are, in part, being phased out and so the score is really even better than is shown here.

When the relevance of the projects was evaluated, 95 percent of the projects had a score or five or better on the question of relevance. It is also interesting to note that the differences between the various parts of the basic energy sciences program; that is between material science, chemical science, et cetera, are not really significant.

I would then conclude that the quality of the basic energy sciences program is high, and that it supports the mission. Another conclusion that was drawn from the results of the review is that the quality of the research at the laboratories and at the universities is comparable.

Let me turn now to the second question. I have so far asked the question, are we doing good research? The answer to that is "yes." The more complicated question is, are we doing the right research?

The basic energy sciences program asks this question continuously and there are a number of ways in which it addresses it. First, there are formal reports commissioned by the Department of Energy. Some of them date back to 1973 and the energy crisis. The Dixie Lee Ray report on the Nation's energy future, advises managers of DOE—or the Atomic Energy Commission at that time—about what research needs to be done in the basic sciences. A much more recent study by the Energy Research Advisory Board, "Federal R. & D. Priorities," appeared in 1981. They examined 49 projects as is shown in my second viewgraph which you may have difficulty seeing. They looked at 49 projects and rated them with three letters, "M", "L", and "S". "M" meaning more money, "L" meaning less money, and "S" meaning the same. Of the 49 projects, only 9 were found to be deserving of more money. Of those nine, three are found in the basic energy sciences program, and two of them in the materials program and the chemical program, both of which are rated "M" are the major sub-programs in the basic energy sciences program.

A third study was made in 1980, called Basic Research Needs in seven energy-related technologies. Another one dealt with chemistry and was a National Academy of Sciences report published in 1981 on "Chemistry Research in the Department of Energy." In 1978, nine workshops were held throughout the country on what materials research to do in the basic energy sciences. All of these reports contain a mass of advice and only part of that, of course, can be followed at any one time. That is one mechanism.

The second mechanism takes place at the national laboratories which carry out about half the research funded by the basic energy sciences program. At the laboratories, the scientists who are funded by the BES program are in intimate contact with the energy technologies. They see firsthand the barriers that prevent some technologies from reaching the marketplace and the barriers that develop as a technology is being carried out.

The national laboratories also employ numerous advisory committees that advise the management about what research to undertake.

Finally, there are formal meetings between the laboratory managers and BES managers every 6 months in which this advice is fed back and effects the direction of the BES program.

Those are two methods of arriving at the question of what to do, they are really advisory methods. Science itself has a certain set of imperatives. The best science is the science that cannot be predicted; that is one imperative. The second one is that scientific research is most productive when the greatest opportunities are seized.

In the fourth viewgraph I show, schematically, how this actually works. Here I describe the opportunities which have been created in the last decade in the field of metallurgy. Metallurgy is a very important area of science for the energy program. We now have a whole array of new micro-analytical tools available for research (I will not bore you with interpreting all these initials they represent very modern and also very expensive devices.) New processes have been developed in rapid solidification of materials and then there are advances in the theory of metals and theory of alloys.

The opportunities that are created in this way result in two things: They result in advances of science which I list on the top, as well as in new applications which are listed below. So, it is important to understand that it is only through the development of new opportunities that a science can progress most efficiently in this age of very competitive research programs here and abroad.

This leads us to four criteria when we ask the question, are we doing the right science? First, "Is the science energy related?" Second, "Does the research exploit the greatest scientific opportunities?" Third, "Does the research address important technological barriers?", Fourth, "Does the research take a broad and long view of the BES objectives?"

Let me expand a little on the fourth point. The only way unexpected and unpredicted science can happen is if the basic energy sciences program takes a broad view and allows its program enough freedom so that new things and totally unexpected things can be found.

I would then conclude that the Basic Energy Sciences program, in the light of these criteria, is indeed relevant; it is relevant in two ways. It expands knowledge, and it produces new applications and new things which are useful in the context of energy development.

Finally, I will address the last question which has to do with the adequacy of Federal support for basic energy sciences.

My story is not too different from that of my predecessors here. In 1983 dollars, the 1970 budget was $265 million, followed by a precipitous drop of about 30 percent to $184 million. The 1983 budget of $261 million in operating money is just about the same as it was in 1970. This is difficult to cope with for two reasons: One is the expanded responsibility of the basic energy sciences program that deals now with a whole spectrum of energy technologies including fission, fusion, solar, conservation, and geothermal, whereas in 1970, the main concern had to do with nuclear energy. The second reason why the program is tightly constrained is that new research opportunities cost a lot of money. New, large devices offer great opportunities to research but they are expensive.

In the next viewgraph I illustrate three classes of devices listed roughly by acquisition costs.

In the multimillion dollar bracket comes the synchrotron light source which is being built at Brookehaven, neutron scattering sources, a combustion laboratory at Livermoore and a high temperature materials laboratory that we have been proposing at Oak Ridge for several years and which is not this year in the President's budget.

In the $1 million class are the surface modification laboratories, accelerators for atomic physics, and advanced electron microscopes.

In the half million dollar class there are things like the nuclear-magnetic resonance probes which are so useful in studying structure and properties of coal and coal liquids, a laser laboratory, X-ray stations at the light sources and vacuum ultraviolet stations for surface research.

The recent history then, conveys a depressing funding picture. In the next viewgraph I show the funding history for basic energy sciences from 1970 to 1983. You see the drop in 1974. The unfortunate thing that happened was that in 1980, 1981, and 1982, the appropriation was lower than the request to Congress. In both 1981 and

1982 that difference was about $17 million dollars in current dollars and has hampered the research program in basic energy sciences.

I would like, therefore, to propose three items for your consideration.

One, that major new facilities that open up opportunities for research be built as part of the policy for funding basic energy sciences. I estimate that this cost would be something like $150 million over the next ten years, or $15 million per year on the average. In a $300 million program which BES now approaches, $15 million invested in major, new facilities in major, new opportunities, does not seem overly ambitious.

Two, that existing and productive facilities should be supported to gain maximum return in the shortest possible time, even as those facilities that are less than frontier are shut down.

Three, that the smaller programs in basic energy sciences, in geoscience, engineering, biological energy, should be brought along faster than the cost of living, commensurate with the problems that confront the energy development in those areas. However, some agreed upon limitation of funding should be arrived at and the funding for those programs should not be on a fast track beyond that limit.

I will summarize. I am happy to report that the state of basic energy science is healthy. With your continuing support we can continue to address the barriers in the technological development for energy, we can obtain new knowledge that will enable us to produce new energy technologies in the future and we can advance the scientific base of the country, which we have to do to stay competitive in the modern world.

Thank you very much.

[The prepared statements of Dr. Zucker follow:]

TESTIMONY BEFORE THE
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
COMMITTEE ON SCIENCE AND TECHNOLOGY
March 25, 1982

My name is Alexander Zucker. I am Associate Director at the Oak Ridge National Laboratory with responsibility for the physical sciences. I appear here today to discuss the DOE program in the Basic Energy Sciences (BES). In accordance with your letter of invitation, I will discuss three topics: (1) the quality of research funded by the Office of Basic Energy Sciences; (2) the relevance of that research to strengthening the energy technology base; (3) the adequacy of the level of Federal support to maintain the excellence of the BES program.

I. Quality of Research in Basic Energy Sciences

Summary. In 1981 the Basic Energy Sciences Program quality was assessed in an extensive peer review. A random sample of 129 projects, out of a total of about 1200 projects, was reviewed in detail by 180 reviewers drawn from the university, industrial, and National Laboratory community. The following conclusions result from the assessment:

(1) The Basic Energy Sciences Program is a well-managed program. It provides high quality science and at the same time it supports the mission of the Department with a well-planned long-range research effort.

(2) The Basic Energy Sciences Program has remained strong in spite of fast-changing demands.

(3) When research at the National Laboratories was compared to non-Laboratory projects, the quality of both groups was comparable.

(4) The review was fair, objective, and insightful.

I elaborate on the summary in the material that follows.

The Basic Energy Sciences Program is the country's major funding source in many areas of physical research. It is particularly strong in materials, mathematics, chemistry, and nuclear sciences, and it is becoming increasingly involved in important areas of energy-related research in biology, engineering and geosciences. Total funding in FY-82 for the Basic Energy Sciences Program is $243 million, with an FY-83 budget request of $283 million. About 3000 scientists are supported in part or wholly by this program and the astonishing number of 5000 publications has appeared in the scientific literature in 1981. A little over half of this work is done at the DOE Laboratories. University scientists perform about 30% of the research and the rest is divided between industrial laboratories and other institutions. The assessment of the Basic Energy Sciences Program thus constitutes a review of what is probably the world's largest integrated basic research program in the physical sciences. No mean task.

The objective of the review was to assess the quality of the management of the Basic Energy Sciences Program, and it took the form of a carefully planned peer review which examined 129 projects out of a total of approximately 1200 projects that BES funds.[1] Peer review continues to be the favored method of evaluating the quality of basic research because the practical payoff is many years away, and hence it is difficult to base funding decisions on such criteria as productivity, economic gain, or customer satisfaction. Peer review is not a foolproof way to evaluate a research program, but that is a subject that has been debated extensively in the scientific community over the past few years, and not an appropriate topic for today's discussion.

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[1] "An Assessment of the Basic Energy Sciences Program", Department of Energy Report (1982).

Nevertheless, peer review remains the current method of choice and once it was settled on the important issues to be addressed were:

(1) the selection of projects to be reviewed;
(2) the mechanism by which the reviewers were to be chosen;
(3) the mechanics of the review process;
(4) the criteria for judging the projects;
(5) the statistical evaluation of both the project selection process and the assessment results.

I address each of these in turn.

(1) Project Selection. The projects selected for review were chosen at random except that some projects funded at levels over $500,000 per year were added to assure that a significant sample of such large money projects was included in the assessment.

(2) Selection of Reviewers. The peer reviewers were selected for their ability to review a particular project, or group of projects, and a serious effort was made to select the best reviewers. Suggestions for reviewers were sought from the National Academy of Sciences, from industrial and National Laboratories, from the BES program managers, from the principal investigators of the projects to be reviewed, and from other knowledgeable scientists. After all these recommendations were received, the final selection was made by members of the staff of the DOE Office of Program Analysis. In order to avoid conflict of interest, preference was given to individuals not currently funded or under consideration for funding by the Basic Energy Sciences Program. Occasionally, when the potential reviewer was overwhelmingly the best choice, he or she was selected in spite of an association with BES. The final distribution by the reviewer's institution is given in Table I for each of the BES program areas.

Table I. Distribution of Reviewers by Institution

Program Area	Industry	Laboratory	University	Other
Materials Sciences	12	11	20	5
Chemical Sciences	14	7	19	10
Biological Energy Sciences	1	0	15	0
Nuclear Sciences	0	7	5	2
Engineering, Mathematics and Geosciences	0	4	17	2
Advanced Energy Projects	7	9	7	6
Total	34	38	83	25

(3) Review Process. Once the reviewers for the project were chosen, panels of reviewers were constituted and each panel met once to review its assigned projects. The panels consisted of three to six scientists qualified to review all of the projects at a general level, but at least one panel member was a true expert in the area of each project to be reviewed. The panel members received in advance of the review meeting a package which contained project descriptions, publications and other project outputs, personal achievements, and peer recognition. At the panel meeting the principal investigator of a project gave a 20- or 30-minute presentation of his program, and ample time was allowed for discussion by the panel members and the principal investigator.

(4) Criteria for Judging Projects. A set of criteria was developed which asked reviewers to assign a numerical rating from 0 to 10 for each project on the following items:

(a) quality of project team;
(b) scientific merit (the importance of the scientific problem being addressed);
(c) scientific approach, techniques, and resources;
(d) productivity.

Because a significant portion of BES-funded research is narrowly directed to support one or another energy technology, and because that influences research strategies, a separate set of criteria for mission impact was included in the evaluations as follows:

(a) ways in which a research project can contribute to the Nation's energy needs;
(b) the importance of the research to the particular technology;
(c) the impact of a project, if successful, on the DOE mission.

Finally, each project was assigned an overall evaluation, again rated from 0 to 10, where the following definitions apply.

Overall Evaluation	Rating
Outstanding project	9-10
A strong project deserving of continuing priority support	7-8
A project with negligible shortcomings	5-6
Good project with deficiencies that should be addressed by BES	3-4
Project with serious shortcomings which warrant close reevaluation	0-2

(5) Statistical Evaluation. Both the selection of projects, and the uncertainties implicit in the sampling process and in the evaluations, were subjected to careful statistical analysis. Throughout the analysis statistical factors were applied whenever averages, proportions, or frequency distributions were calculated and confidence limits were established for all conclusions. While much of the statistical analysis is highly technical and inappropriate for this presentation, it is of interest to point out that the concensus among the reviewers was amazingly close. For example, the overall evaluation distributions within the panel averaged ± 0.43 on a ten point scale.

The entire assessment process was developed by the Office of Program Analysis. The Director of this office is Dr. George Y. Jordy and the principal person responsible for the assessment was Dr. John R. Powers. It is my judgment that both of these gentlemen, as well as the capable staff working with them, did their work very well. In the planning stages and during the actual execution of the assessment, Powers and Jordy consulted extensively with members of the scientific community both within the BES funding structure and outside it. Discussions were held with a group of senior consultants in several workshops at which first, plans were discussed, second, the progress of the assessment was reviewed, and finally, the significance of the final results was evaluated and analyzed. The participants in these workshops were:

Dr. Martin Blume	Brookhaven National Laboratory
Professor D. Allan Bromley	Yale University, President AAAS
Dr. A. M. Clogston	Bell Laboratories
Dr. John K. Galt	Sandia National Laboratories
Dr. Michael V. Nevitt	Argonne National Laboratory
Professor Robert W. Parry	University of Utah; President, American Chemical Society
Dr. David A. Shirley	Lawrence Berkeley Laboratory
Professor Charles P. Slichter	University of Illinois, Physics Department
Dr. Alexander Zucker	Oak Ridge National Laboratory.

The conclusions of the workshop at the end of the assessment were that the assessment was carried out intelligently, capably, and with due regard to the sensitivity of the scientists involved. The general perception by all was that it was a fair review, and it is to the credit of all involved that the assessment was regarded as insightful and objective.

Assessment Results and Conclusions

The overwhelming result of the assessment was positive. The BES program is well managed, and the scientific projects that it funds are sound. Table II summarizes the overall results.

Table II. Proportion of BES Projects Above Selected Rating Thresholds

Program Area	Percent Above Threshold		
	5	7	9
Materials Sciences	.90	.63	.04
Chemical Sciences	.88	.57	.11
Biological Energy Sciences	.93	.64	.07
Nuclear Sciences	.96	.64	.00
Engineering, Mathematics and Geosciences	.97	.49	.22
Advanced Energy Projects	.91	.73	.00
Overall BES	.91	.59	.10

It is apparent from Table II that 91% of the BES-funded projects are above the level-five threshold; i.e., none of these has any important shortcomings. Fifty-nine per cent of the projects lie above the level-seven threshold (they are therefore strong projects deserving priority support), and about 10% are outstanding. Only about 9% of the projects fall below a 5 rating and thus have important deficiencies that require serious attention from BES officials. A significant fraction of the low-graded projects were in fact being phased out or undergoing important changes in direction well before the assessment was undertaken. No significant quality difference is apparent for the six BES program areas.

The results of the analysis of the impact of the research on the DOE mission are equally gratifying. Ninety-five per cent of the BES projects received ratings of 5 or above in measuring the importance of the research to a particular technology, and 90% are so rated for the significance of the impact of a project, if successful, on the DOE mission.

A more detailed analysis of the data showed several interesting results.

(1) The large-dollar projects, defined as funded above $500,000 per year, rated generally higher than the projects funded at lower levels. However, some highly rated projects were found at all funding levels.

(2) Comparison was made between the projects carried out at National Laboratories and those carried out in universities and in industry. For this purpose, projects between $100,000 and $350,000 per year were selected in order to arrive at a fair comparison. Table III gives the results of this analysis.

Table III. Comparison of Laboratory and Non-Laboratory Projects in the Funding Range $100 - $350 Thousand per Year

Rating Threshold	Percent of Projects Above Rating Threshold	
	Laboratory	Non-Laboratory
5.0	.97	.78
7.0	.82	.51
9.0	.08	.15

(3) A comparison of ratings of recent and long-term projects was made. Long-term is defined as initiated in FY-74 or earlier. Table IV gives these results for non-Laboratory projects and it seems clear that the recent projects are superior to those that have been carried out for a long time.

Table IV. Non-Laboratory Projects: Comparison of Recent and Long-Term Projects

Rating Threshold	Percent of Projects	
	Recent	Long-Term
5.0	.90	.58
7.0	.51	.36

On the other hand, long-term Laboratory projects are of the same quality as recent Laboratory projects, Table V.

Table V. Laboratory Projects: Comparison of Recent and Long-Term Projects

Rating Threshold	Percent of Projects	
	Recent	Long-Term
5.0	.94	.95
7.0	.75	.74

The difference between the results of Table IV and Table V may be due to the fact that projects at Laboratories receive continued management review and evaluation, whereas those carried out at universities, for example, are pretty much left to the principal investigator's own judgment since the academic system, quite appropriately, provides no technical supervision by department chairmen, deans, etc. Furthermore, if one takes into account projects that are being phased out and subtracts them from the sample, the difference between Table IV and Table V becomes much smaller.

A citation analysis was carried out on the papers published by BES-funded investigators. Citation analysis is a paper-by-paper evaluation of how many times that paper was referred to by other authors in other scientific publications. In the words of the very careful analysts: "The average impact of quality of the papers by the BES investigators, as determined by citation analysis, was measured in comparison to distribution of citation frequency for journal articles published in the same subfields and with the same number of citation years. When adjusted for subfield and citation year peculiarities, the 2.0033 citations received by the 2648 BES papers are a significantly larger amount than the average number expected on the basis of comparison citation distributions for all journal articles published in Standard Citation Index-covered journals with at least one U. S. author." What this statement means is that in the evaluation of over 2500 BES papers, the BES-supported scientists are well above the national average of all journal articles in the same areas of science.

II. Relevance to the Energy Technology Base

I now turn to the question of the relevance of the BES program in supporting a vigorous energy technology development in the United States. While Section I of my Testimony responded to the question, "Are we doing good research?", in this Section we ask a more subtle question: "Are we doing the right research?" This question is seriously and continually addressed by the BES Program Office, by the Laboratories that carry out about one-half of the research, by the engineers and managers responsible for the energy technology programs in DOE, and by the scientific community at large. I present here some of the formal and informal mechanisms used to monitor the current status of BES research and to plan research direction for the future, with particular regard to the question of "right" research.

(1) Formal reports are commissioned by DOE which wholly or in part review the need for and effectiveness of basic science as related to energy technology development. These reports go back to the so-called Dixy Lee Ray Report[2] prepared at a time when the oil embargo first made the energy crisis palpable. The latest of these reports was published in November 1981 as "Federal Energy R&D Priorities"[3] by the Research and Development Panel of the Energy Research Advisory Board.

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[2] D. L. Ray, "The Nation's Energy Future", WASH-1281, USAEC, December 1973.

[3] "Federal Energy R&D Priorities", Report of the Research and Development Panel, Energy Research Advisory Board, November 1981.

(2) The Office of Basic Energy Sciences and its partners in DOE who manage fossil energy, conservation, fusion or some other aspect of energy technology, hold workshops and commission reports that examine the capabilities of the BES program to provide support in the areas of need in the technology programs. Two years ago seven reports[4] were generated in this way, and their results constituted a significant portion of the material used by the R&D Panel of ERAB to arrive at its recommendations.

(3) The managers of BES programs at the multipurpose laboratories where much of the research is carried out are, of course, highly motivated to see that the "right" research is carried out. It is precisely at the laboratories where the BES program comes in contact with the technologies, where the short-range hurdles as well as the long-range problems of a technology become painfully obvious, and where the BES program can be molded to respond to near-term problems and to forecast as well as prevent difficulties in the long term. At the laboratories the mechanism is often through direct interaction between the scientists and engineers working on the technology and those working on the supporting basic research. Most laboratories also have external advisory committees who are asked to address this question and to provide guidance to the laboratory Directors. The results of these interactions, as well as the recommendations of advisory bodies, are then cycled back to the BES officials during formal semiannual meetings as well as in more informal ways.

(4) The Office of Basic Energy Sciences commissions special studies either by organizations like the National Academy of Sciences[5] or by

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[4] "Basic Research Needs in Seven Energy-Related Technologies", DOE/ER-0060, USDOE Report (1980).

[5] "Report of the Ad Hoc Panel on DOE Chemistry Research Committee on Chemical Sciences", National Academy Press, Washington, D. C. (1981).

ad hoc committees to examine this issue. An example of the latter was a series of nine workshops held in 1977 that examined the entire materials program and made recommendations to the BES managers as well as to the scientists themselves.[6]

My own conclusions are that there is almost too much advice, but that people involved in both the management and execution of BES programs are indeed paying attention. One must realize that within the funding limitations of the program and the availability of scientific talent, one can not do everything. Furthermore, it would be a miracle if occasionally some mistakes were not made. Nevertheless, the overwhelming evidence from all the advisory machinery and its numerous reports is that the program is supportive of the energy technologies, and that the balance between its parts is reasonable.

The BES program can not be formulated merely on the basis of technology needs and the capabilities of the program to fulfill them. Any good long-range program must also perform research in areas where the connection to energy is but dimly perceived, where judgment and selection are based more on the imperatives of science than on the pull of the technology. One of these imperatives is that research should be concentrated in the areas of greatest opportunity. Science thrives and is most productive when a field has just opened up, perhaps because a new technique has become available, perhaps because some significant discovery has opened new vistas, or perhaps because a new theoretical formulation makes the perspectives clearer and the research more powerful or innovative. Another imperative of basic research is that we must not always insist that we know where it leads us. "The best science is unpredicted science", says Edward Teller. The totally new, the breakthrough in science, is also the unanticipated.

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[6] Materials Science and Engineering, Vol. 35, (1978).

So much for strategy, what about results? The sole indicator of the "rightness" of a research program is the "rightness" of the results. In the case of BES one expects two kinds of results.

First, the research produces new knowledge about nature, knowledge that is in its own way every bit as fundamental as the findings in elementary particle physics, or astrophysics. Who, for example, would say that the discovery of efficient long-distance electron transport in photosynthetic processes is any less fundamental than the existence of a certain color quark? Or that the coexistence of magnetism and superconductivity in certain classes of superconductors is a lesser puzzle than the behavior of pulsars?

Second, the research may produce results that can be readily applied to solve an energy-related problem, or it may produce totally new materials or processes that find application in another sphere of technology or science. Here I would include such recent discoveries as the invention of completely new alloys with astonishing high-temperature properties, the synthesis of new ceramics with much improved resistance to fracture, new processes for environmentally benign recovery of uranium, or a deeper understanding of chemical processes that take place when coal is liquefied.

When all of these considerations are taken into account, the following criteria for BES research emerge:

(1) the science is energy related;
(2) the research exploits the greatest scientific opportunities;
(3) the research addresses important technological barriers;
(4) the research takes a broad and long view of the BES objectives.

It is my experience that the BES program does in fact do the "right" research. Its projects are energy related without being stifled by requirements for relevance or short-term payoff. The program exploits scientific opportunities, and in fact generates many such opportunities from its own ranks. The combustion facility, the synchrotron light source, and the new emphasis on high-temperature materials all present new opportunities, and provide scientists with new tools that are already producing startling new fundamental knowledge or results which can be applied to overcome technology barriers. I can not here review the "rightness" of the entire program; let me just illustrate what I mean by a few examples. Take, for instance, the great triad of materials sciences: solid state physics, metallurgy, and surface science. There is overwhelming consensus that materials science is strongly coupled to energy and in Figure 1 I show how one member of the triad, metallurgy, exploits new opportunities offered by modern instrumentation and modern scientific concepts to develop materials of the greatest potential promise for high temperature and other applications, while at the same time opening up ever expanding vistas in understanding fundamental properties of metals or ceramics. The results of this research strategy are impressive. Advances in metallurgy have transformed this science in the last decade. The DOE-related applications include development of low-swelling alloys without which neither fast fission breeders nor fusion reactors could become commercial. Diagrams for surface science and coal chemistry (Figure 2 and Figure 3) display similar results. Both new knowledge and practical results abound; in all three of these - metallurgy, surface science, and coal chemistry - BES is the major U. S. funding source for fundamental research.

The fourth criterion to "take a broad and long view" deserves special mention here. It is for this reason that BES support of university-based research is so important. University research coupled with the widespread use of major laboratory-sited facilities by university faculty and graduate students assures that the research is not excessively applied, and brings into the BES fold scientific ideas from very diverse backgrounds. The interrelationship between the university community and the National Laboratories works. At the Oak Ridge National Laboratory alone 450 person-years of research were carried out by university guests: faculty, students, postdoctorals, etc. This compares with the total scientific and technical staff at ORNL of about 1500 people.

III. Adequacy of Federal Support for BES Program

Before I discuss the adequacy of support required for BES to discharge its function, I want to review briefly the funding history of the last decade (Figure 4). In 1970 the BES operating budget stood at $265 million (1983 dollars) [nearly equal to the FY-83 request of $261.3 million]. There followed a steep 30% decline to $184 million (1983 dollars) in FY-74, and a subsequent slow increase to $226 million (1983 dollars) in FY-82. In 1970 the BES program, or its equivalent in the Atomic Energy Commission, supported only the nuclear option and maintained a few national facilities; the rest of the program was devoted largely to very basic and therefore very long-term research.

Much has happened in the intervening years. The consciousness of the importance of adequate energy to life in America has reached all levels of our society. The Federal government reorganized the institutional arrangements twice and is proposing yet a third reorganization. The mission of basic energy sciences was broadened from the narrow focus on nuclear energy to include an entire panorama of energy options: fossil, fusion, conservation, solar, and geothermal, as well as nuclear.

A second, less obvious, development has been the increasing cost of research. It isn't simply that basic research costs escalate with inflation or even a little faster than the normal cost of living indicators. The very nature of physical science has changed over the last decade. Materials research now requires such expensive large experimental devices as synchrotron light sources, high-flux reactors, and implantation devices to probe ever subtler properties of matter and its surfaces. Atomic physics requires heavy-ion accelerators every bit as powerful, and as expensive to build and operate, as nuclear physics. Chemical research requires costly NMR machines for the study of coal- and shale-related research, and the searching and precise tools for analysis far surpass both in sensitivity and relative cost the analysts' tools of the early seventies. It may appear simply that inflation has hit research harder than other sectors of society. But in fact the new tools are infinitely superior, they make some research possible that could not be done before, they provide more results, more precisely, and at lower cost per result than was possible twelve years ago. It is only because of the increasing efficiency of modern research equipment that BES programs have been able to expand at all into the total energy domain, and do a creditable job there. To illustrate this point, Table VI lists some examples of new research tools in three broad categories, according to the capital costs for acquisition.

Table VI. Examples of New Research Installations or Instruments for Three Approximate Levels of Acquisition Costs

Multimillion	~1 Million	~.5 Million
Synchrotron Light Source	Surface Modification Laboratory	NMR (Nuclear Magnetic Resonance)
Neutron Scattering Source	Atomic Physics Accelerator	Laser Laboratory
		X-ray Station at Light Source
Combustion Laboratory	Advanced Electron Microscope	
High Temperature Materials Laboratory		VUV Station at Light Source

It is a tribute to the dynamism and foresight of the management of the BES program that they have supported these new facilities. It is a tribute to this Committee that it continues to see the value of such new developments. It is precisely the acquisition of new facilities that has made the basic energy sciences so productive, so adventurous and, I believe, so successful in developing new knowledge as well as new applications. Even so, the funding picture over the past few years is depressing. Figure 4 illustrates this point. Since FY-79 the Congressional appropriation has been significantly below the Department's request to Congress and in FY-81 and FY-82 the shortfall has been about $17 million (current) in each year. This at a time when the broadened mission of BES was about to be implemented in new programs, when the new facilities placed greater demands on the dollars available, and when support for university-based research was being disproportionally increased.

We all agree: more needs to be done with less money. The normal recourse to reprogram projects into either more relevant areas or into more effective endeavors allows considerable latitude, but it is not nearly enough. There remains today a large reservoir of important and often critical research that the basic energy sciences program is unable to explore. Scientists understand the economic problems of this country and they know that we can not afford to do everything that we know should be done. Let me therefore propose the following points:

(1) That major new facilities be built, even as old ones are shut down, and that this entails a minimum investment in new facilities of about $150 million over the next ten years, averaging about $15 million a year. Fifteen million dollars a year for new major facilities does not seem an excessive number to assure the success of a $300 million a year research program.

(2) Active major facilities should be supported to extract maximum results from them in the shortest possible time. This requirement accounts for much of the increase in materials sciences in the FY-83 budget.

(3) Geosciences, engineering, and biological sciences should be brought along faster than cost-of-living to make their funding truly commensurate with the problems they must tackle. This fast-track treatment should aim at an agreed upon ultimate funding level.

In summary, I would ask the committee to provide sufficient funds to exploit new opportunities in science, to build or acquire new tools, and to provide BES with resources to expand into critical new energy-related fields. In this way the BES program can best serve the Nation by providing new knowledge that will have a major impact on the country's energy supply and consumption sectors, on the economy and well-being of its citizens. I hold this view from the perspective of a research laboratory, a place where the action is, where daily there are new developments in science, and where two or three times a year I am privileged to see truly important scientific accomplishments born and sent into the world.

FIGURE CAPTIONS

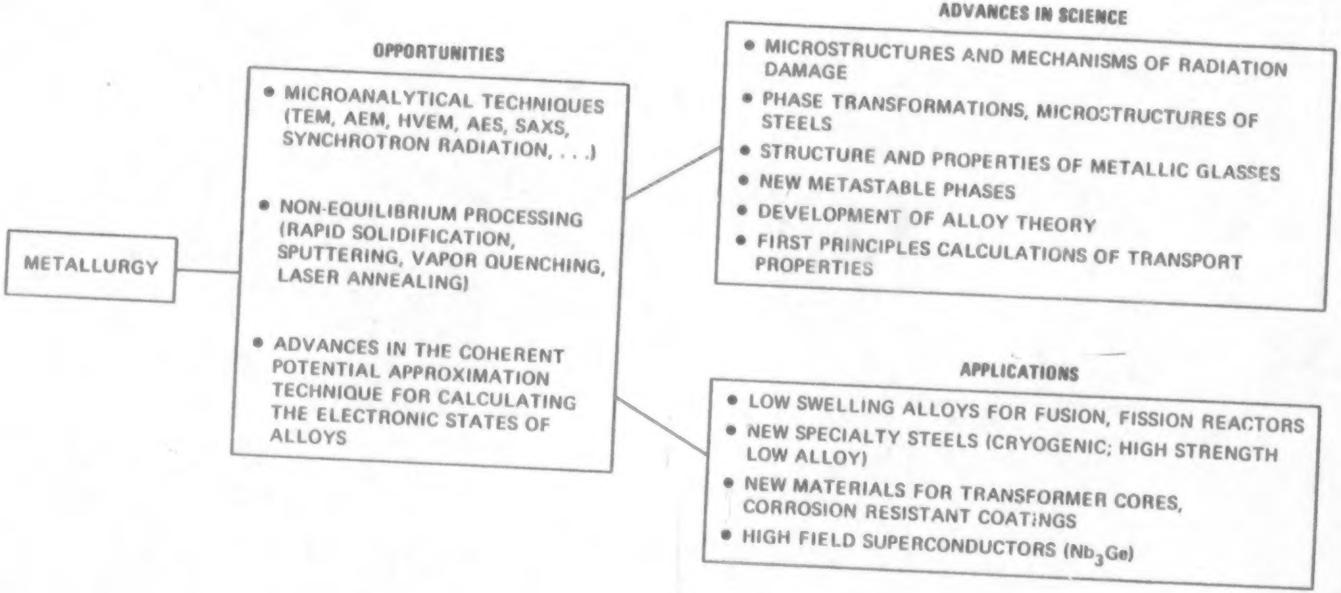
Figure 1. Development of research in metallurgy: significant advances in sciences as well as applications to energy technology are based on exploitation of newly opened opportunities in analytical techniques, processes, and advances in theory.

Figure 2. Development of research in surface sciences: significant advances in sciences as well as applications to energy technology are based on the explosive development of new equipment in the last decade, as well as on the deepened understanding of properties and behavior of surfaces.

Figure 3. Coal chemistry and catalysis: significant advances in science and their applications are based on the realization of the importance of the Nation's most plentiful fossil fuel, and on the increased understanding of chemical structure, chemical reactions, and catalytic surfaces.

Figure 4. BES funding history of the last decade in 1983 dollars: the differences between the request to Congress and the final appropriations in FY-81 and FY-82 have been about $17 million (current dollars in each year). This shortfall has caused serious difficulties in the development of the BES program in view of its broadened mission in support of long-range options of many energy technologies.

ORNL–DWG 82-10548



METALLURGY

OPPORTUNITIES

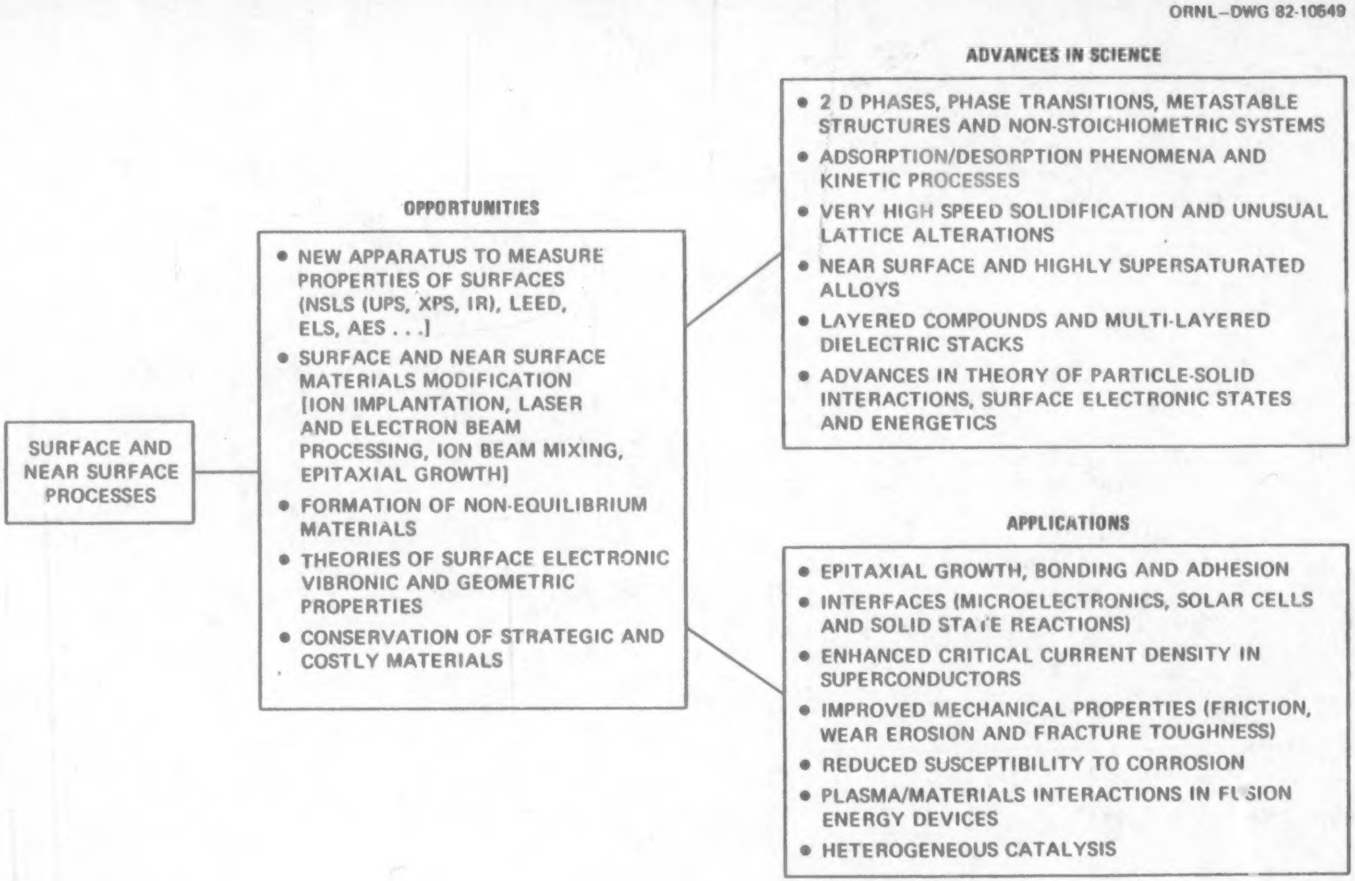
- MICROANALYTICAL TECHNIQUES (TEM, AEM, HVEM, AES, SAXS, SYNCHROTRON RADIATION, . . .)
- NON-EQUILIBRIUM PROCESSING (RAPID SOLIDIFICATION, SPUTTERING, VAPOR QUENCHING, LASER ANNEALING)
- ADVANCES IN THE COHERENT POTENTIAL APPROXIMATION TECHNIQUE FOR CALCULATING THE ELECTRONIC STATES OF ALLOYS

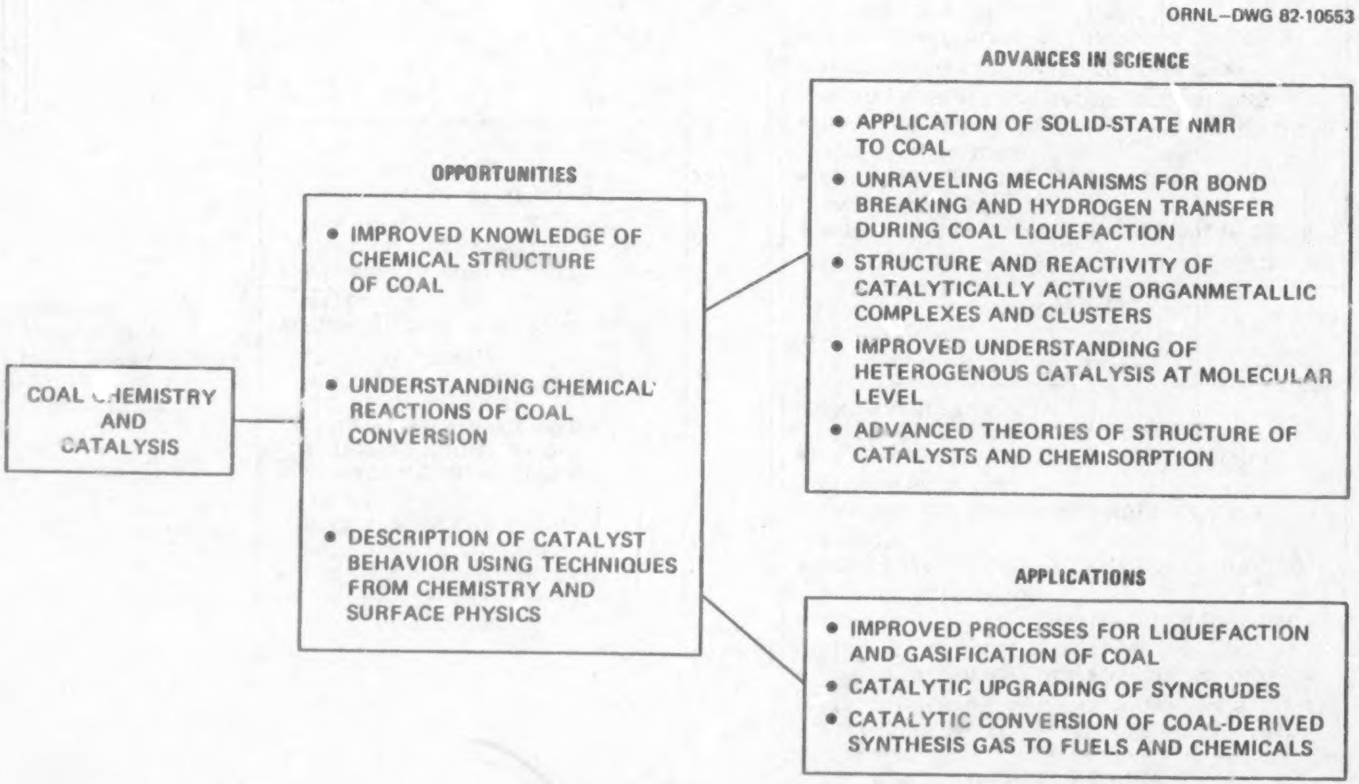
ADVANCES IN SCIENCE

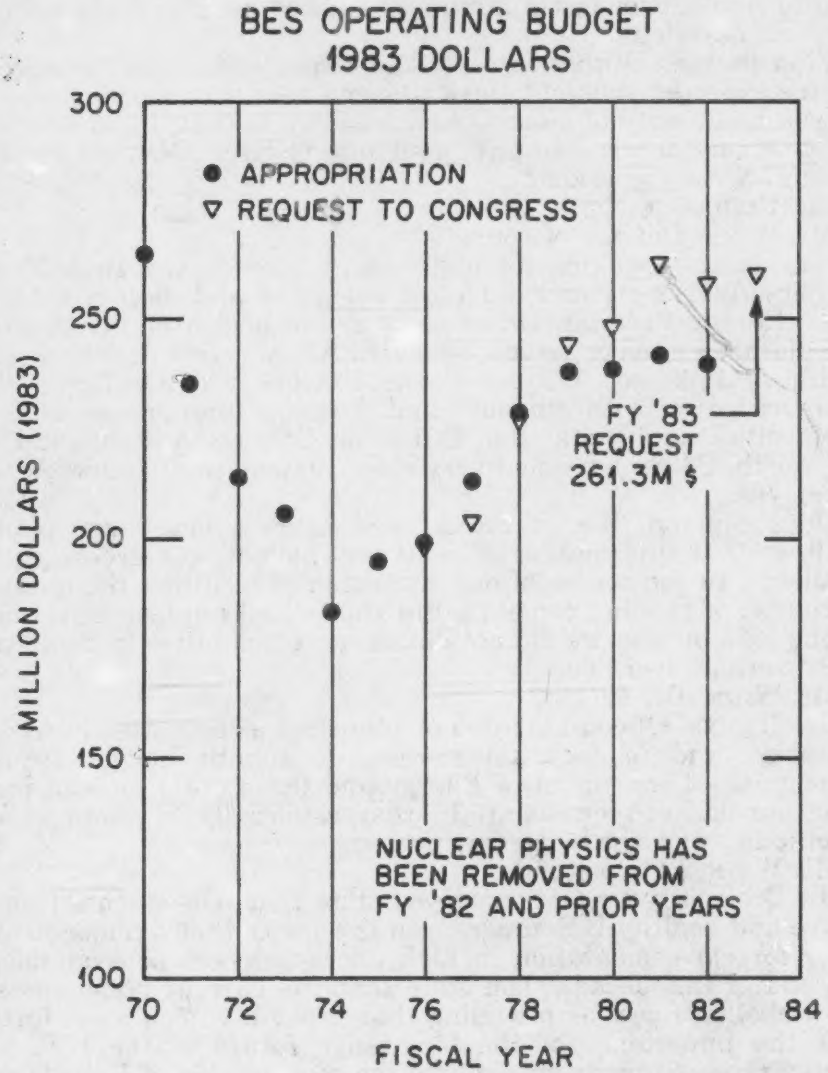
- MICROSTRUCTURES AND MECHANISMS OF RADIATION DAMAGE
- PHASE TRANSFORMATIONS, MICROSTRUCTURES OF STEELS
- STRUCTURE AND PROPERTIES OF METALLIC GLASSES
- NEW METASTABLE PHASES
- DEVELOPMENT OF ALLOY THEORY
- FIRST PRINCIPLES CALCULATIONS OF TRANSPORT PROPERTIES

APPLICATIONS

- LOW SWELLING ALLOYS FOR FUSION, FISSION REACTORS
- NEW SPECIALTY STEELS (CRYOGENIC; HIGH STRENGTH LOW ALLOY)
- NEW MATERIALS FOR TRANSFORMER CORES, CORROSION RESISTANT COATINGS
- HIGH FIELD SUPERCONDUCTORS (Nb_3Ge)


ORNL–DWG 82-10649
SURFACE AND NEAR SURFACE PROCESSES
OPPORTUNITIES
• NEW APPARATUS TO MEASURE PROPERTIES OF SURFACES (NSLS (UPS, XPS, IR), LEED, ELS, AES . . .]
• SURFACE AND NEAR SURFACE MATERIALS MODIFICATION [ION IMPLANTATION, LASER AND ELECTRON BEAM PROCESSING, ION BEAM MIXING, EPITAXIAL GROWTH]
• FORMATION OF NON-EQUILIBRIUM MATERIALS
• THEORIES OF SURFACE ELECTRONIC VIBRONIC AND GEOMETRIC PROPERTIES
• CONSERVATION OF STRATEGIC AND COSTLY MATERIALS
ADVANCES IN SCIENCE
• 2 D PHASES, PHASE TRANSITIONS, METASTABLE STRUCTURES AND NON-STOICHIOMETRIC SYSTEMS
• ADSORPTION/DESORPTION PHENOMENA AND KINETIC PROCESSES
• VERY HIGH SPEED SOLIDIFICATION AND UNUSUAL LATTICE ALTERATIONS
• NEAR SURFACE AND HIGHLY SUPERSATURATED ALLOYS
• LAYERED COMPOUNDS AND MULTI-LAYERED DIELECTRIC STACKS
• ADVANCES IN THEORY OF PARTICLE-SOLID INTERACTIONS, SURFACE ELECTRONIC STATES AND ENERGETICS
APPLICATIONS
• EPITAXIAL GROWTH, BONDING AND ADHESION
• INTERFACES (MICROELECTRONICS, SOLAR CELLS AND SOLID STATE REACTIONS)
• ENHANCED CRITICAL CURRENT DENSITY IN SUPERCONDUCTORS
• IMPROVED MECHANICAL PROPERTIES (FRICTION, WEAR EROSION AND FRACTURE TOUGHNESS)
• REDUCED SUSCEPTIBILITY TO CORROSION
• PLASMA/MATERIALS INTERACTIONS IN FUSION ENERGY DEVICES
• HETEROGENEOUS CATALYSIS


ORNL–DWG 82-10553
COAL CHEMISTRY AND CATALYSIS
OPPORTUNITIES
• IMPROVED KNOWLEDGE OF CHEMICAL STRUCTURE OF COAL
• UNDERSTANDING CHEMICAL REACTIONS OF COAL CONVERSION
• DESCRIPTION OF CATALYST BEHAVIOR USING TECHNIQUES FROM CHEMISTRY AND SURFACE PHYSICS
ADVANCES IN SCIENCE
• APPLICATION OF SOLID-STATE NMR TO COAL
• UNRAVELING MECHANISMS FOR BOND BREAKING AND HYDROGEN TRANSFER DURING COAL LIQUEFACTION
• STRUCTURE AND REACTIVITY OF CATALYTICALLY ACTIVE ORGANMETALLIC COMPLEXES AND CLUSTERS
• IMPROVED UNDERSTANDING OF HETEROGENOUS CATALYSIS AT MOLECULAR LEVEL
• ADVANCED THEORIES OF STRUCTURE OF CATALYSTS AND CHEMISORPTION
APPLICATIONS
• IMPROVED PROCESSES FOR LIQUEFACTION AND GASIFICATION OF COAL
• CATALYTIC UPGRADING OF SYNCRUDES
• CATALYTIC CONVERSION OF COAL-DERIVED SYNTHESIS GAS TO FUELS AND CHEMICALS


BES OPERATING BUDGET
1983 DOLLARS
● APPROPRIATION
▽ REQUEST TO CONGRESS
FY '83
REQUEST
261.3M $
NUCLEAR PHYSICS HAS
BEEN REMOVED FROM
FY '82 AND PRIOR YEARS
300
250
200
150
100
MILLION DOLLARS (1983)
70
72
74
76
78
80
82
84
FISCAL YEAR

Mr. WINN. Thank you, very much, gentlemen. The Chairman had to step out for just a minute and asked me if I would start my series of questions.

First let me say that I do not think that anyone on the subcommittee expected you gentlemen to come here this morning and tell us you had plenty of money. As a followup to that, I just wondered whether any of you had any input into OMB or DOE for the 1983 budget? Were you asked?

Mr. FESHBACH. No.

Mr. WINN. Did any of you?

Mr. DRELL. Speaking for high energy physics, the High Energy Physics Advisory Panel did meet regularly and discuss with the High Energy Program Office of DOE the health of the program. We did have a major review by a HEPAP subpanel under the leadership of Professor Trilling at the University of California. This was carried on with guidance and frequent interchange with the DOE office and with the Office of Science Advisor and Dr. Keyworth. So we have had very close rappore and discussion with them, yes.

Dr. FESHBACH. We, of course, had made a long-range plan in 1979, so that information of what we thought was necessary was available. In the course of our discussion of facilities, the question, of course, of funding came up. But this actual number here that is in the 1983 budget, we did not discuss as a committee in detail with these various agencies.

Mr. WINN. Dr. Zucker?

Dr. ZUCKER. Of course, I do not represent a committee here. Laboratories, and in fact universities, do submit budget requests through the Department of Energy and these are a form of input. Once our budget requests are in, that, essentially, is where we stop interacting with the budgetary process.

Mr. WINN. I understand.

Dr. Drell, in your testimony you state that it is essential for an active and healthy U.S. program in the 1990's that a major new facility for experimentation in high energy physics be available by the end of this decade. You state that the current plans envision the Isabelle project as providing that capability. You state further that the importance of the long-range future of the U.S. high energy research program requires the construction of Isabelle. You also point out that successes in the superconducting magnet development at Brookhaven have greatly increased technical confidence in the project. It would appear then that Isabelle is the only possibility of meeting these needs since we are told that it takes 10 years after conception to complete the construction of a high energy physics facility. It would appear then that if the high energy physics program wishes to maintain leadership in the world community, all projects and all programs in the high energy physics activity must participate as a united group to assure the completion of the Isabelle project.

Would you care to enlarge on that a little bit?

Dr. DRELL. Yes, thank you, Mr. Winn.

We have struggled, as a community, very hard in our planning with the Isabelle project and we have looked in detail both at the estimate of funding needed to go from today to an operating accelerator with a modest complement of equipment. We have tried to fit that in at various budget levels in making the recommendation that I have.

Fact one is that with a modest complement of equipment, the completion of Isabelle still requires, in fiscal year 1982 dollars, a total funding of $500 million—the biggest project we have. We have asked, "what would happen to the rest of our program if we squeezed that program in in order to complete Isabelle by the end of this decade?" We looked at the impact, first on utilization. The national community strongly emphasizes that over the past few years the utilization level of our existing accelerators, the opportunities for our present physicists—our university community—have been inefficiently and painfully inadequate. The need to restore some strengthened operations, some strengthened utilization was, in our judgment, the No. 1 priority that had to be attended to. We have to operate what we have invested in already to keep the present program alive and from attriting as it is now doing. Then we looked at the status of completion of scientific progress at the energy saver program at Fermilab, which, during the 1980s, is the base of the program.

We asked, "what would happen to these if, at the same time, we were to go ahead with Isabelle?" Isabelle for the 1990's, as I have said personally—and my own view is very strong on this—must be a major facility. It is essential. When we did our long-range planning, as we have over the past 7 years now and we undertook both the Fermilab and the Isabelle construction, we were given budget guidance that corresponds to this dotted line in figure 1 in terms of constant value dollars. We reaffirm that with a budget that is very close to that level, we have the right program. We feel confident that with the recent technology advances Isabelle is on the right track. However, trying to squeeze utilization, the Fermilab program, the technology of accelerator R. & D., particularly as represented for the future by the SLC collider, we cannot do it all. It was just a scientific judgment that for the program in the 1980's to remain competitive, healthy, and retain a base for the future, we could no longer squeeze into it a program of the scale of the present Isabelle project and, therefore, it is our judgment that the best we can do—and I am not happy about this—is to scale down the project. It is to go back to the R. & D. program, with accelerator R. & D. in 1983 and in 1984 if necessary, to see how effective, how major, how exciting a facility we can achieve at a lower budget figure than $500 million. It will mean a quality program but certainly a less competitive one if we stay at the present level. That was a difficult judgment and it represents the community judgment.

Mr. WINN. I think it is a difficult judgment that the funding of Isabelle and whether we ought to continue with that or not has also been very controversial both in this committee and in the full committee. It is something we are still taking a look at and still listening for it.

Along the same general line, I just wondered why it is desirable to maintain all three DOE high energy physics centers. That is, the Stanford Linear Accelerator Center, Fermilab, and Brookhaven? That is a kind of a dirty question to ask you.

Dr. **Drell**. No, it is not. I assure you that we have tried our best to come to grips with the program insofar as what is a healthy base program. Let me be clear. HEPAP has been very effective in coming up with community views; we are not a unanimous community on difficult decisions. We have managed, as a united community, to come forward. We are exploring an unknown region, Mr. Winn, and we do not know where we are going to run into gold, or whatever it is. When the Stanford Linear Accelerator Center was first created 20 years ago—it was authorized in the late 1950's and created 20 years ago—the view that electron beams, as opposed to proton beams, would be a very fertile approach-probing substructure and elementary forces, was one that was not the majority view—and yet it proved to be a very rich way of going at the unknown. What is more, we learned how valuable it is, as we are groping in the darkness of an unknown future, to have different kinds of probes. Sometimes a clue from one turns out to be the trigger that suggests using another type of accelerator for a certain experiment or research program. So there is a very important synergistic effect.

Mr. **Winn**. You would not think there was any duplication, overlapping, there that could be combined?

Dr. **Drell**. The number of accelerators over the last 10 years has decreased very markedly. I think we had, a little more than a decade ago, about 10 operating accelerators. We now have three at DOE labs and one at Cornell. The feature of the Fermilab Tevatron is that with relatively few interaction regions, it allows us to go to the very highest energy with proton beams and proton-antiproton colliders. The Isabelle feature that is unique is that it allows us to go to high energy—not quite as high as Fermilab—but with an interaction rate which we expect to be, given our present technical confidence, anywhere from 1,000 to 5,000 times higher than the interaction rate at the Tevatron I. That means that rare events, where on occasion we have found the richest surprises, can be studied. They are not accessible to study by low luminosity.

Meanwhile the Stanford complex has pioneered the electron beam so that we have electrons and positrons rather than protons. If I can make a crude analogy, when you hit a target particle with electrons it is like a jeweler probing with a fine instrument because we know how the electrons forces operate and they are relatively weak. When you hit with a proton beam it is like a sledge hammer. To find out what is going on inside of a truck, sometimes you want the jewelers pincers and sometimes you want a sledge hammer. They give very different clues; debris versus scattering patterns. Our experience is our best guide here and so far we have found that having both the electron and the proton probes, both the high energy and the high luminosity, has been crucial.

We have been very careful to design a program that complements and does not, in any way, duplicate the European program. We talked very extensively with our European colleagues and our Japanese colleagues now that they have become big-time players in this field, as their budget shows, They are on our advisory committees and we are on theirs. It has been of highest priority to the community not to unnecessarily duplicate and none of our future planning has in it what I would consider, as a scientist, duplication.

Mr. WINN. That is good to hear because we are at that stage, because of the economy, where we have to check everything we can and try to cut it out. Obviously we are getting ready to do it in DOD as well as DOE and NASA and everybody else.

Mr. DRELL. It is best for our scientific ability. Let me say that we have been conscious of that. There is something suspicious about three laboratories preserving themselves in three regions of the country. We criticize ourselves. We are always pushing each other on that point and I think it is an honest judgment that HEPAP, which is representative of all regions and styles of physics, has come pretty well to grips with this problem. We have said that we do need Isabelle because of its unique capabilities as a third experimental approach.

Mr. WINN. Dr. Zucker, in your testimony you indicate that a little over a half of the work in basic energy sciences program was done at the DOE laboratories with about 30 percent performed at the universities and that the remaining undertaken at industrial laboratories and other institutions.

Do you believe that this would be considered a proper balance of support for the program or would you recommend some additional trends for the future, particularly with the budget restrictions?

Dr. ZUCKER. On the whole I would say that is a proper balance.

In the last few years the university support has grown and I think it has reached about the right level. I would say that at this point and from here on the current ratio of fund division between national laboratories, universities and other institutions, including user facilities, is about right.

Mr. WINN. Sort of along the same line, significant increases have been recommended in recent years in biology, engineering, and geosciences activities. Do you believe that these programs would duplicate the work already being supported by other Federal agencies? Geoscience, for instance, at the Department of Interior? How closely are BES projects coordinated with similar projects funded elsewhere? What type of a system do we have to coordinate those?

Dr. ZUCKER. Let me address geoscience as an example. The whole energy picture begins and ends with the earth, so to speak. All our energy resources come from the earth, all the garbage that results at the end goes back into the earth. By "earth" I mean the ground, air and water. So, if you take a look at the overall question of energy, it is clear that knowledge about the earth is an important integral piece of the whole action.

A very important reason for the difficulties that nuclear energy finds itself in right now is precisely the lack of attention paid to that sort of thing in the past 20 years.

So I would say that the Department of Energy needs to have its own program in geosciences because its needs are different from the Geological Survey needs, for example.

Now let me address the second part of your question. I do not see duplication between DOE programs in engineering, geosciences, and biological energy really for two reasons. The funding for research in the Department of the Interior is going down and the Geological Survey, which is one of our sterling organizations in science is in difficulty. They are not able to address those things that have to do with waste, with nuclear waste, with other kinds of waste. They are not able to address the question of resources, geothermal energy, the questions that have to do with the atmosphere and water as repositories for various kinds of wastes. The Department of Energy needs to do that and the Department of Energy needs to build a long-term base of expertise.

I see no reason to worry about duplication because the scientists who work in these fields interact in many ways. They interact through professional meetings and we have good contacts with the Department of the Interior and their managers. We know what each of us is doing. One thing we avoid like the plague is duplication; we do not want to duplicate.

Mr. **Winn**. Well that is very assuring because we do not always find that in every Government agency.

Thank you, very much.

Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, Mr. Winn.

Dr. Drell, in following up on some questions that Mr. Winn addressed regarding Brookhaven, you stated in your testimony that even if the Isabelle project at Brookhaven cannot continue, there is still a need for a new major facility for the 1990's. Should that new major facility, whatever it is, be built at Brookhaven?

Dr. **Drell**. I have no preconceived prejudice or notion on that. I think if we, under the budgetary constraints, come back with a project of reduced scope relative to the present Isabelle project then I do not know what that project should be. I think the most urgent need now is the accelerator R. & D. including, in particular, the support for that at Brookhaven to come up with a plan and for the community to have a chance to come up with its plans and to review them so we can make our best bet. It obviously would be very attractive if we were able to use a tunnel that is sitting there. I would like to think that that might be a solution personally, but there is no community judgment. I do not know, in my own mind, what that project will be.

Mr. **Fuqua**. What are some of the useful options for the Isabelle complex at Brookhaven that would be less expensive than the original design?

Dr. **Drell**. I know that there are some of my colleagues studying this intensely, and on this issue I ask for special forbearance from you since I am a theoretical physicist who is far away from being a machine designer. There is a lot of very intense work in two directions. One is on a somewhat reduced scope proton-proton storage ring and another is an electron-proton storage ring. You can have electrons colliding with protons just as well as protons with protons. This is another kind of physics and has been very productive in the past. The crucial questions are,what maximum energy of collision? What interaction rate can one actually achieve given the technology of the magnets that have been developed both at Fermilab and at Brookhaven? And we want to see the physics potential of electron-proton collisions at the cost and at that site. I honestly do not know the answer, but I do know that we urgently need to continue supporting accerlator R. & D. to get the answer to that question as soon as possible.

Mr. **Fuqua**. We have a Federal investment of about $130 million thus far in the Isabelle project. My concern is how can we best protect that investment?

Dr. **Drell**. I think that would have to be part of the decision in addition to the scientific judgment that will be coming from the community.

Mr. **Fuqua**. Which of the generation of facilities, Isabelle, Tevatron I, Tevatron II or the Stanford Linear Collider, has the potential to accommodate the largest number of experiments?

Dr. **Drell**. The potential for accommodating the largest number of experiments over a long run would be Isabelle and Tevatron II [protons on fixed targets]. The point is that a facility or experiment which aims high-energy particle beams into a fixed target leads to a much higher interaction rate because a fixed target has so many atoms in it—so many nuclei in it—with which you can collide that the interaction rate is much higher than when two beams go through each other. The great charm of the colliding beam machines is that all the energy of both beams goes into the collision so that you reach much higher energies. In terms of instrumentation and research areas, the Isabelle would support a larger community than Tevatron I and the Stanford Linear Collider. When we talk about the 1980's program, we are saying the complex of the Tevatrons [Tevatron's fixed target and proton-anti-proton storage ring], the collider at SLAC plus PEP, and the remaining program at the AGS, can support a full and rich program. Whereas, in the 1990's those accelerators will have—unless there is a major revolution or our physics ideas are shattered even more than I expect them to be—those accelerators will have done their major work and will not be able to provide a richness of new frontier probing and broad support that I think Isabelle, were it available in 1990, would provide.

Mr. **Fuqua**. Why is it so necessary to begin the formal construction of this Stanford Linear Collider in fiscal year 1984 such that the R. & D. phase can be completed in fiscal year 1986? How much does the SLC serve the general high energy physics community?

Dr. **Drell**. The SLC, in its initial phase as supported by HEPAP, will have one experimental area and two detectors or two major groups—consortia—being able to work at it. It will support a finite but limited community.

The reason that it has been supported so strongly by the by HEPAP subcommittee and by HEPAP is twofold. As an accelerator R. & D. project it is developing a new technology which we think will open the avenue to a long-term future research frontier. The point in simplistic terms is that the storage rings get expensive roughly as the square of the colliding energy. The reason is that a storage ring, in order to have an interaction rate which is commensurate with finding new discoveries in a finite time, must have the two beams that are circulating—you must store them and have them circulate and collide roughly 100,000 times per second.

Mr. **Fuqua**. Why is 1984 the driving factor?

Dr. **Drell**. I am saying, first, to get on with the technology for the future.

The other reason we see is the specific opportunity that the Stanford Collider, at 100 GeV, will be the first and perhaps the most powerful machine for studying the properties—perhaps even discovering—of the quantum of the weak interaction which has been hypothesized in the theory that unified electromagnetism and weak interaction physics. We are desperate to see that we are on the right track. In particular there is major scientific excitement at prospects of discovering how many neutrinos there. This number colors our whole idea of the formation of the universe—we are dependent upon those results. This is a machine at the $100 million construction cost which, in terms of that physics, promises to be operating several years ahead of a billion Swiss franc machine that is already in the beginning stages right now; the so-called large electron-positron or LEP machine at CERN. This is, in a sense, the United States way of keeping the ball in play at $100 million versus a much larger investment, using conventional technology in Europe. There is no doubt that we not only want the answer fast but we want the answer.

Mr. FUQUA. We keep hearing that the Tevatron project I at Fermi is to be rescoped. How is this effort going to affect the project's total cost and completion date? Also, will the project be able to fully utilize the $18 million it has requested in fiscal year 1983?

Dr. DRELL. I believe that the Tevatron I program needs the $18 million in 1983 to stay on schedule. I know that the technical subpanel review that was part of the HEPAP long-range planning did come in with a figure for the Tevatron I which is somewhat higher than the present budget. I think that Dr. Reardon's subpanel was speaking of a number which was in the range of $70 million to $80 million, as opposed to being in the 40 millions. I do not have myself enough detailed analysis of the budget in all of the outyears ahead to know how that fits in or whether that means a stretchout in time of Tevatron I versus Tevatron II, versus the Stanford collider, or how we maintain operations. I do not want to be irresponsible, but sitting over there is Dr. Sandweiss, my successor as Chairman of HEPAP. After 8 years of struggling with my colleagues in trying to help the gallant office of Dr. Wallermeyer and Dr. Leiss of DOE with these questions, I am now able to smile and say, "he has got his work cut out for him this year." I will be helping him face this issue as a member of HEPAP but I cannot answer your question.

Mr. FUQUA. I have some more questions, but I will yield at this time to Mr. Volkmer.

Mr. VOLKMER. Dr. Feshbach, has the Nuclear Science Advisory Committee had any discussions about limiting the scope of the DOE nuclear physics program?

Dr. FESHBACH. We, of course, heard from the DOE program offices of Dr. Ritter and Dr. Leiss with respect to that 1982 project. We were very, very disappointed and we do not believe that that budget will maintain a nuclear physics program at a competitive level with Europe and Japan, nor will it assist our sciences industry, energy, and so forth very much. If it maintained at this level I think that it is a signal that the program is in serious, serious trouble.

Mr. VOLKMER. How do you think the reduction in the heavy ion nuclear physics' operating expenses impact the major facilities at the national labs, such as Lawrence, Berkeley, Oak Ridge, Brookhaven and Arsonne?

Dr. **Feshbach.** Well in different ways but all, one must say, unhappily. At Brookhaven there will be a phase out of the tandem which is one of the more advanced machines. At Berkeley, the superhilac, which is a very useful machine to this day, will be used only for injection into the bevalac which is a high energy machine. It means a substantial reduction in the utilization of that machine. Almost a hundred percent reduction inasfar as the range of nuclear physics is concerned.

Mr. **Volkmer.** Which machine was that?

Dr. **Feshbach.** Superhilac.

At Argonne, I just received a letter from the Argonne people who tell me in some detail—unfortunately I did not bring it with me—what the impact would be on their Atlas program which, as you know, you have authorized in the past. It is simply going below threshold for utilization and operational results.

I have not discussed the matter regarding Oak Ridge with the Oak Ridge group but Dr. Zucker could probably respond to that part of it.

Mr. **Volkmer.** Could you respond to that part Dr. Zucker?

Dr. **Zucker.** Yes, sir. Oak Ridge has the most modern machine which is just coming on the air now, the Holifield Heavy Ion Laboratory, and the reduction in funding or the limitation in funding will seriously impact our ability to make this machine available to the users—the 300 users from various places, universities and so on. It curtails our ability to run our cyclotron more than 20 or 30 hours a month so it is a very serious problem for us. We get our highest energies by injecting the tandem into our cyclotron. That is currently the highest energy heavy ion obtainable in this country and abroad. So we are sitting on the world's best potential facility and we are seriously limited in the amount of time we can use it seriously impacts us.

Mr. **Fuqua.** Dr. Feshbach, Mr. Winn touched on this issue in regard to high energy physics facilities. Let me now ask about duplication in the nuclear physics research facilities and their capabilities. Is there much duplication and is it desirable?

Dr. **Feshbach.** There is duplication in the sense that there are some facilities which can do what other facilities can do. It is a very different matter, though, than it is in the particle physics game. It is like asking, how many electron microscopes should there be in biology? Should there be only one electron microscope? The really relevant issue is what is the range of phenomena that can be studied and what are the trained personnel available to do it. The point there is that there is a very broad range of studies to be made. There are many nuclei and many phenomena to be investigated and the way the field has progressed is by making that kind of overall look and then getting in and understanding how the thing works. So, although you may find two or three facilities that have the same sort of capability, the scientific opportunities will exhaust very readily any one of them. At the present moment in fact, we are short on capability with respect to scientific opportunity and this would get worse with the present budget. The question is what science can be done and we need those machines to do the science. The same experiment is not being done at three different places.

Mr. **Fuqua**. Dr. Zucker, you outlined an ambitious program for basic energy sciences, particularly advocating a systematic addition of large, modern facilities to enhance the capability of the basic energy sciences program. Is there a long-range plan to determine which facilities are needed and, if not do you think there should be one and how should it be developed?

Dr. **Zucker**. I believe that the basic energy science people have a long-range plan. It is not formal in the sense that anybody can see it.

If BES does, in fact, embark on an ambitious program such as I outlined, I think they should develop a structure that is somewhat analogous to the HEPAP and NSAC structures where there is a scientific consensus reached about what are the highest priorities. It is more difficult to organize this in the basic energy sciences because the disciplines vary broadly. The people who can recommend what chemical facility needs to be built, are not the same people who can recommend what materials facility or engineering science facility needs to be built. But, I believe that can be managed. There are enough people around who have knowledge in several disciplines and there are ways of getting experts together and reaching a consensus. Again, I believe the scientific community can develop such a plan if the money becomes available.

Mr. **Fuqua**. Dr. Zucker, are any barriers to industry participation in the basic energy sciences program being removed?

Dr. **Zucker**. I think the picture is really looking much better in a formal way.

The national laboratories are now in position to accept industrial users and people to come in from industry and do research using the major equipment.

Our modern surface research equipment, has perhaps 30 or 40 industrial users per year who come in and use it for their own basic research.

The problems of proprietary information are being overcome as well. When a facility is designated a "user" facility then industry can come in and not reveal proprietary information they do their experiment, provided they pay the full costs of operating that facility.

Just very recently, in the last few weeks, we have received authorization at Oak Ridge to accept industrial funds for research at the laboratory, up to $50,000 based on the signature of the Director. That cuts out a lot of red tape, I can assure you of that. The last approval before this action was taken took 18 months. It took us that long to get industrial funding at the laboratory. So, things are looking up.

Mr. **Fuqua**. Maybe you have some examples of industry participation?

Dr. **Zucker**. Yes, we have Du Pont coming into Oak Ridge—they are actually funding research at Oak Ridge at about $60,000—to use the solid state nuclear magnetic resonance apparatus and look at the structures on coal, I believe with the idea that DuPont understands that at some point oil and gas will run out, and they are looking to coal as a chemical feedstock. This is not proprietary, research program incidentally.

Industrial users of the surface modification facility include Exxon, Motorola, Westinghouse, Bell Labs, IBM, INCO, Corning Glass, United Technologies, Hughes, and Texas Instruments; a veritable honor roll of high technology industry. At the small angle neutron scattering facility operated jointly by the National Science Foundation, the Department of Energy and the Oak Ridge National Laboratory, it is noteworthy that three industrial concerns—Du Pont, Firestone, and Eastman Kodak—are doing proprietary research, while many other industries participate in the more common nonproprietary mode.

When we did not get funding for high temperature materials laboratory, we decided to go ahead with the high temperature materials program anyway and this program is now in full force and INCO—International Nickel—is preparing some samples which we are then looking at with our equipment. There is another industry interested in some of our new ceramic materials that we are just developing. The whole area of high temperature materials and development of alloys and ceramics is very interesting. We had a meeting of industrial vice presidents last year where we generated a lot of interest and enthusiasm. That is a good sign.

There is also a bad part to be perfectly candid. I think you would probably want to hear the whole business. It is slow. It is very difficult somehow to make industry aware that the national laboratories' facilities exist and that they are there for their use. I think that is a matter of education. It is a matter of unremitting visits and discussions with industry to assure them, "yes, this is a laboratory that you support with your tax dollars. If you have need for it then come and use it." That is slow and for my taste it is too slow.

Mr. **Fuqua**. Thank you.

Mr. Volkmer?

Mr. **Volkmer**. No questions.

Mr. **Fuqua**. I want to thank you Dr. Drell, Dr. Feshbach, and Dr. Zucker for being here this morning. You have been very, very helpful. We have some additional questions that we will want to submit to you for answers in writing. Mrs. Bouquard has some questions for you, Dr. Zucker. I do not think you will find them too difficult to answer. We want to thank all three of you for your contribution and help in our consideration of DOE's basic energy programs.

Dr. **Drell**. Thank you, Mr. Chairman.

Dr. **Feshbach**. Thank you, Mr. Chairman.

Dr. **Zucker**. Thank you, Mr. Chairman.

Mr. **Fuqua**. The committee will stand adjourned until 9:30, Tuesday morning.

[Whereupon the subcommittee adjourned at 11:11 a.m.]

STANFORD UNIVERSITY

Stanford Linear Accelerator Center

Mail Address
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Stanford, California 94305

April 16, 1982

The Honorable Don Fuqua, Chairman
Committee on Science and Technology
U. S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D. C. 20515

Dear Congressman Fuqua:

Enclosed are the replies to the written questions from members of the Subcommittee that you sent me relevant to my testimony of March 25.

Sincerely yours,

Sidney Drell

Sidney D. Drell
Professor and
Deputy Director

SDD:br

Enclosure

1. "Which of the three DOE high energy physics laboratories currently serves the greatest number of outside users?"

Fermilab currently serves the greatest number of outside users. One way to measure the service to the user community by DOE high energy laboratories is in terms of dollars spent by the university user groups from their budgets performing their experiments at the laboratories. By this measure of the total dollars contracted to university experimental user groups by DOE plus NSF, 55% is spent at Fermilab, 30% at SLAC, and 15% at Brookhaven. The relatively low percentage of money spent at Brookhaven reflects the fact that there is not now a new frontier facility in operation at that laboratory.

"Should the ability to serve many outside users be a major criterion in providing continuity of laboratory support?"

This is, indeed, an important criterion to be included in developing an overall balanced and healthy national program. However, there is a still more important criterion and that is the ability to explore new science on new frontiers. In order to help maintain a healthy involvement and support of outside users in the research programs of the national laboratories each has its own scientific policy committee and organized user group. The scheduling committees at the individual laboratories also judge the merits of proposals in terms of the strength of the scientific team, as well as the excitement of the physics. The priority concern must remain the quality of the research and the importance of the new physics being explored.

2. "Are the DOE and NSF high energy physics programs truly complementary?"

The DOE and NSF both serve important and different roles in the national high energy program, but I would not call them complementary. They both complement and supplement one another. The DOE, of course, operates the three major national laboratories, while the NSF supports the Cornell program which is built around the CESR storage ring. DOE and NSF support comparable numbers of university user groups at the more than one hundred institutions involved in high energy research.

"What do you see as the future roles of NSF and DOE, respectively, in high energy physics research?"

I believe NSF will continue to have an important function in the future in its support of the national program for several reasons:

- There are questions of judgment in selecting which of the research proposals to support from many applicants. There is no infallible wisdom in making this choice, and it is therefore very important that research proposals have a chance to be reviewed by two independent mechanisms. Thus DOE and NSF serve as a check on each other's style, judgment, or prejudices, if you wish.
- The fluctuations of the budgetary process make it important that there be more than one funding agency. A diversity of funding

sources can serve a balance-wheel function in time of need. I note that the proportion of support from the NSF has decreased over the past decade from close to 8% to little more than 7% of the national program relative to DOE. I hope this trend will be reversed in the future.

3. "The high energy physics community has ambitious plans for new, large and expensive research facilities, but we are faced with indefinite and severe budget constraints. What plans are you making for international cooperation in building and operating large facilities in the future?"

International cooperation in the building and operation of future major facilities has been discussed extensively in the high energy physics community and in the HEPAP deliberations. In particular, the International Committee for Future Accelerators, ICFA, has three American representatives, appointed with the concurrence of the chairman of HEPAP.

It is my feeling that, as we look ahead into the decade of the 1980's, the primary emphasis will be on regional and international exploitation of national accelerators. HEPAP's recent review has verified that during the past decade there has been a healthy and well-balanced exchange of scientists between U.S. and Western European accelerators. In our planning of new facilities for the decade ahead we have included physicists from overseas to help insure that our programs complement rather than duplicate one another's. We are continually discussing, especially at ICFA, the movement toward international cooperation and the actual planning and operation of large facilities, along with the costs and problems that will arise thereby. We also recognize the vital importance for a strong and fruitful cooperation to the existence of comparable strengths in the participating regions. This can become a problem in the future, as indicated in my testimony (Figure 2) if the support of the U.S. program continues to decline vis-a-vis Western Europe.

In addition to scientific cooperation, official cooperative arrangements have been made by the U.S. with the Soviet, Japanese, and Chinese governments for scientific exchange, and in the case of the Chinese, for assistance in the design and construction of China's first planned high energy electron-positron storage ring.

4. "In your view does the FY 1983 DOE request for high energy physics reflect a proper balance between physics research, facility operations, facility construction, and long range R&D for the future generation of facilities?"

At the level of the President's budget for DOE, I believe that the budget for the FY 1983 program is correctly balanced. It emphasizes utilization of existing facilities, completion of the Tevatron project, moving ahead with the SLC collider research at Stanford, and accelerator R&D for the future, emphasing in particular a major national facility needed for 1990 and beyond. The national program would be stronger if the budget were returned to the level of the OMB/DOE 1978 agreement, as indicated in my testimony, so that construction of the ISABELLE project as currently defined could continue.

OAK RIDGE NATIONAL LABORATORY
OPERATED BY
UNION CARBIDE CORPORATION
NUCLEAR DIVISION

POST OFFICE BOX X
OAK RIDGE, TENNESSEE 37830

April 28, 1982

Honorable Don Fuqua, Chairman
Committee on Science and Technology
Suite 2321
Rayburn House Office Building
Washington, D. C. 20515

Dear Mr. Fuqua:

Enclosed are my responses to the written questions that arose from the testimony on basic energy sciences at the Subcommittee on Energy Development and Applications hearing on March 25, 1982.

I found the hearings most interesting and I hope that the testimony and the answers to the questions may be of some use when you and your Committee prepare the Authorization for the Basic Energy Sciences Program. I am happy to say once again that BES is a great program and that all the evidence indicates that your support of it has been well justified.

Sincerely yours,

Alex Zucker
Alexander Zucker
Associate Director

AZ:c

Enclosure

Response to Questions of the Members of the Subcommittee on Energy Development and Applications Committee on Science and Technology

Alexander Zucker
April 28, 1982

Question 1. How do you see the difference in roles between the universities and laboratories in the Basic Energy Sciences Program?

I first address the role of the multidisciplinary National Laboratories in the Basic Energy Sciences Program. The principal and most obvious mission of the BES Program is to provide a knowledge base which in the long term will supply information to advance the energy technologies. These technologies must be broadly defined to include the whole spectrum beginning with identification and extraction of resources, the conversion process that makes energy available, the disposal of waste which is present in any energy process, and the assurance that the entire cycle be carried out in an environmentally acceptable and safe manner. In the Laboratories, connections are made between scientists working on fundamental questions and the technologists involved in energy development. In a synergism that has thus developed the basic scientists gain firsthand information about problems in the technology, be they materials problems of the first wall in fusion, the effect of CO_2 on global air circulation, or the safe disposal of nuclear wastes in geologic formations. With their firsthand knowledge and personal acquaintance with the issues of energy technology, the BES-supported scientists can formulate their programs in a way that will supply knowledge and data required to overcome barriers in technology development. Or conversely, the engineers responsible for a technology can consult freely with their scientific colleagues to learn about the newest scientific findings and thus gain access to a worldwide scientific data base which they can reach in no other way except through personal communication with knowledgeable experts. In sum, then, one role of BES programs in the National Laboratories is to provide a synergism between science and technology which first, advances the science, second, identifies areas for scientific inquiry, and third, provides insights and data for the solution of technical problems.

The establishment and operation of large user-oriented facilities available to university and industrial communities in the United States is a second role the multipurpose laboratories play in the BES programs. In many areas of science large facilities are becoming increasingly essential to advance a discipline. Large facilities make entirely new investigations possible and they provide modern equipment and modern techniques for the study of matter. But they are more than that. They are also the centers of lively research where the ferment of new ideas and new results is easily observed. The interaction between scientists, both those working in the basic sciences and those working on applied problems, is a unique and valuable component found at large facilities. The BES-Laboratory involvement in such facilities is essential for two reasons. First, the creation of such a facility requires expertise and deep knowledge of the scientific areas to be investigated as well as strong engineering and support functions; second, a user facility must have an active on-site research group which will develop appropriate instrumentation, data collection systems, and other ancillary devices and keep the facility modern and in top working condition.

A third role of the Laboratories has to do with the institutional nature of BES research programs. There are many areas of science in which the Department of Energy must maintain large-scale or long-term continuing research and create an institution which will have both a corporate memory and clear sense of purpose for this research. Examples of this are the study of radiation damage of structural materials, the study of neutron cross sections related to fusion, or the structure of coal and coal derivatives. The time constant for research in energy technology is measured in decades and it is important that basic research which provides the results that undergird the technologies be carried out in a way that matches the long-term nature of the technology. Universities, with some exceptions, are not well matched to this requirement. National Laboratories, on the other hand, are organized to undertake large programs, long-term research, and multidisciplinary research. It was shown in the recent BES assessment review that the National Laboratories are able to carry out long-term basic research investigations without loss of quality and without loss of vitality in the program.

The fourth important characteristic of the multipurpose laboratories in this connection is the ability of the Department of Energy to access this community and in times of crisis call on it to provide information rapidly or solve problems that appear crucial and that require a significant effort in one or more scientific disciplines. Very often it is the scientists supported by the BES programs who are asked to respond, and who do so with a real sense of urgency and involvement. Again some examples might be useful. When the fast breeder reactor first exhibited the swelling of stainless steel due to irradiation by neutrons, the then Atomic Energy Commission turned to the National Laboratories to find a solution to a problem that threatened the future of the breeder. A solution was found and various low swelling alloys are now candidates for both fast breeders and fusion reactors. More recently, in the Three Mile Island accident the activity from escaping radioiodine was lower by a factor of 7,000 than was predicted by some models. The BES researchers quickly found a reason for this in the thermodynamics of complicated water-iodine-cesium mixtures at high temperature that explained the mystery and gave the Department of Energy and the Nuclear Regulatory Commission confidence that at least this aspect of the accident was well understood. Other examples could be cited but let me just restate the general conclusion that the BES programs at the Laboratories are a resource to the Department of Energy which can be quickly mobilized and has in many instances shown itself to be both responsive and successful.

Even as the National Laboratories fulfill the various roles I have listed above, it is important to bear in mind that the BES programs at the National Laboratories are a wellspring of scientific information in the United States. Over the years, the high quality of scientists at the Laboratories, together with the first-rate facilities and support organizations, has resulted in an especially productive scientific enterprise. By any analysis of productivity, excellence or innovation, the BES groups at the Laboratories are at least equal to any scientific cadres found in the world. New ideas, new results, new theories, are vivid proof of the creativity of the programs at the Laboratories and their effectiveness in research. New knowledge and new ideas after all are the raison d'etre of the BES programs and the Laboratories fulfill that mission in full measure.

I turn now to the Basic Energy Sciences programs at the universities. These, of course, are no less important than the programs at the Laboratories and they appropriately have a somewhat different flavor. BES research at universities is still concerned with science related to energy. But at universities the conditions are less constrained. In many cases the scientists will attack a problem in quite unconventional ways. This often results in brilliant new insights and has in the past created whole new fields of inquiry. At universities research is largely governed by the necessity to provide thesis problems and long-term continuity is therefore less likely. This shortcoming is well compensated by the fact that thesis research very often uncovers totally unexpected phenomena and that the mixture of young energetic minds and the drive to complete a thesis can be a potent one indeed. I suspect, therefore, and this has been borne out by some recent studies, that some of the most daring and some of the most innovative results in basic science are the results of university-based research funded by BES. There is, of course, another dimension to the BES programs at universities: it is the need to educate young men and women in this country in the sciences related to the development of energy technologies. After their education is completed these scientists will either carry on the university teaching and research tradition, enter industry, enter government service, or join National Laboratories. Their education depends crucially on a healthy and aggressive BES program in the universities.

Question 2. There are several smaller programs in Basic Energy Sciences; for example, in geosciences, engineering, and biological energy. What do you think are the ultimate appropriate levels for these efforts and do you think that they should be funded at the expense of the more established programs in materials research, chemistry, or nuclear science?

The small new programs in BES - geosciences, biological energy, engineering sciences - tackle very important areas of energy-related research. Geosciences, for example, is a field related to the recovery of resources and disposition of waste for all energy technologies. The study of the atmosphere and the oceans is closely coupled to the question of climate change due to CO_2 enhancement, and the study of the earth's crust is important in predicting the existence of gas and oil resources as well as in the disposal of radioactive waste. Biological energy represents some of the greatest hopes for the development of liquid fuels from renewable resources. A deeper understanding of biological and biochemical processes could vastly enhance human well-being with a minimum of environmental or health effects. Engineering science research in this country is deplorably underfunded. Almost all advances now come as a consequence of near-term development projects and very little effort goes into a long-term understanding of things like multi-phase flow, behavior of structures under stress, or long-term advances in instrumentation science. My own guess is that the appropriate levels of funding for these areas are as follows: geosciences and biological energy, $20 million each (FY-83 $); engineering sciences, $50 to $60 million (FY-83 $). I believe that these funds are commensurate with the problems that need to be attacked. The reason for the high cost of engineering research is in part due to the great diversity of engineering problems that face energy technology development, and also the inherent expense of building engineering research devices. They are big, they need elaborate instrumentation, and they are costly. While it is my firm belief that these programs should grow, I can not advocate that they grow at the expense of programs in materials research, chemistry, and nuclear science. The latter are well established, productive, and extremely important both to the development of energy in the U. S. and to the progress of science. Any tampering with their funding pattern would result in very serious dislocations in what are universally acknowledged to be extremely successful programs. The net result of reprogramming could damage the entire BES Program in a way that would take many years to repair.

Question 3. The trend is to establish more user facilities at the laboratories. What are the responsibilities of the laboratories in setting up user facilities and do you think that the universities and industry are taking advantage of these facilities?

The user facilities at the National Laboratories constitute some of the most important assemblages of new technology and scientific manpower in the United States. As I indicated in my prepared testimony, they create opportunities for pioneering scientific research and produce results at less cost and faster than could be obtained in any other way. In the establishment of the user facilities, the multiprogram laboratories play a most important role in a way which is not always appreciated by the user. I will describe chronologically what actually happens in establishing a user facility, what is required to create, design and build it, and then operate it for the benefit of a broad scientific community.

(1) The first and crucial element in establishing the user facility is the idea. Somebody, almost always at a Laboratory, either has an idea to create the user facility or responds to a perceived scientific need that often surfaces in many places simultaneously. The result may be a light source, a high temperature materials laboratory, a combustion facility.

(2) Once the idea is conceived, it is reviewed by the Laboratory and, if judged to be sound and promising, the next step is to evaluate the interest of the scientific community. This involves workshops, studies, presentations, meetings, reviews organized and carried out by the Laboratory, and dissemination of the idea to the appropriate scientific community at laboratories, universities, and industry. Feedback from the community is important at this stage and frequently modifies the original ideas.

(3) At the same time as the Laboratory seeks to evaluate community interest, another group begins a conceptual

design. Technical questions must be resolved and now the user community must be more intimately involved. The design, after all, has to conform to the needs of the users and this will only happen if the users are heavily involved in the conceptual design process.

(4) Following closely upon the design comes a cost estimate. The Laboratory's experience and expertise in estimating and minimizing construction costs comes into play. In these days of stringent budgets, serious errors in cost estimating can lead to grievous results that can impact a whole field of research. Laboratories therefore bear a great responsibility to produce reliable cost estimates.

(5) The next step is to write a proposal. Here again the Laboratory takes the lead and, with the aid of potential users, produces a document that includes a strong scientific justification for the new facility to present to the scientific community, to the funding agencies, and to the Congress. Writing a good proposal document is no trivial task. It takes time and effort by the best scientists and by the best engineers.

(6) Once the design, cost estimate, and proposal are completed, there ensues a long process known as the search for funding. Many people need to be convinced that the proposed facility is indeed worthwhile, that it is better than some other competing facility, and that it is designed in a way that will fulfill its function. Again the Laboratory bears much of the burden of the work in obtaining funds.

(7) Now we assume that funds are appropriated by the Congress and construction can begin. The Laboratory's responsibility is to see that the facility is built on time and within budget. Unforeseen technical problems have to be overcome. Unexpected and unpredictable changes in the labor market or the economy are no excuse for cost overruns or delays. Here again experience and dedication of Laboratory staff are indispensable.

(8) The facility is now complete. It has to be run for the benefit of all users, and it has to be operated safely and with particular emphasis on reliable and reproducible operation.

(9) Users are a very diverse group. Once the facility is operating it becomes the Laboratory's responsibility to assure their satisfaction, to make sure that time is allotted on the basis of scientific merit, and to see that the users have the right apparatus when they need it.

It is easy to see that establishment of a user facility at a Laboratory requires a great deal of work, high quality performance, scientific and engineering expertise of a high order, and a good deal of diplomatic skill.

The question whether universities and industry are taking advantage of user facilities is more difficult to answer. I suspect a study of this question could be undertaken, but I will here give my own impression. I believe universities are taking advantage of facilities to the extent to which funds for research are available, and funds for graduate students and postdoctoral support can be obtained by the university. Industry is another matter. A number of industries are in fact using user facilities at the National Laboratories. It is my feeling, however, that there is room for growth here and that industrial scientists could benefit more from user facilities, and that the Nation as a whole would be the better for it.

Question 4. There are several areas for which DOE's Basic Energy Sciences and NSF interface and overlap. Is this overlap desirable? Are the respective DOE and NSF subprograms well coordinated? What do you see as the future roles of NSF and DOE, respectively, in basic research?

I am not in a good position to discuss the overlap between DOE's basic energy sciences program and the NSF-sponsored research because I am not terribly familiar with the NSF programs. While I do not have firsthand knowledge about all the steps DOE and NSF take to avoid unproductive overlap, I do know that the BES staff meets regularly with their NSF counterparts. In several areas the cooperation between NSF and DOE-BES is very effective. For example, the use and management of the several synchrotron light sources is a result of such interactions; so is the NSF-funded National Center for Small-Angle Scattering Research located at the Oak Ridge National Laboratory that makes use of the High Flux Isotope Reactor as its source of neutrons.

To my knowledge there is no danger of duplication created by the two funding agencies. The scientific community is structured in such a way that overlap and duplication are almost always avoided and when there is overlap it is usually for a solid scientific reason. The future role of the National Science Foundation and the Department of Energy in the basic sciences is to my mind quite distinct and separate. The Foundation is primarily the creature of the universities. It was created to maintain a high level of scientific strength in the Nation's universities, it funds small projects preferentially, and provides a good deal of support for graduate student research. Its role is broad and it impinges on energy only when the scientific motivation drives it that way. The DOE Program on the other hand is more directed. It selects areas of science that have an impact on energy or that may in the future have some connection with it. DOE research is often concentrated in larger contracts than is the case with the NSF. It tends to support more group efforts than single investigators. While its direction is in part guided by the scientific community, it is also subject to the needs of the country in the broad field of energy. Some of the material that I have included in my answer to Question 1 is also applicable as a response to this question.

Question 5. DOE's Basic Energy Sciences subprograms are currently organized according to scientific disciplines. Is there any merit to organizing them according to interdisciplinary problem areas?

Many of us have pondered the idea of organizing the basic energy sciences according to problem areas rather than according to scientific disciplines. Both have advantages and disadvantages. As it stands, the organization by discipline has several advantages. It provides a peer structure within which the excellence of the research can be judged, it provides a body of literature, and it provides a worldwide community in which the programs are imbedded. There is furthermore a parallelism with the structure in university faculties, and it makes interactions between DOE program managers, National Laboratories, and universities simpler and more effective. The drawback is that the BES Program may have difficulty in mobilizing around a problem that involves several disciplines. To some extent this has been overcome by what I call reintegration at the Laboratory level. That is, when a problem of importance to energy surfaces the Laboratories reorganize their effort around the problem until it is solved and then revert back to the disciplinary organization. An example of this sort of thing is the currently active field of nuclear waste disposal which involves materials sciences, chemical sciences, and geosciences. It is relatively simple at a Laboratory to create task forces which attack the question in many ways and provide the kind of solutions that can be useful and timely. Most of the problem areas in fact lie within one discipline. For example, high temperature materials involves metallurgy, ceramics, solid state physics, corrosion, and structure studies. All of these are part of the materials sciences program of BES and while the effort is truly multidisciplinary it is still within one division. The same argument can be made for surface science, alloy design, coal chemistry, catalysis, etc.

Organization according to discipline thus provides better quality control, fast response to new multidisciplinary problems, and a correspondence to the worldwide scientific community. Many of these advantages would be lost in an organization that is problem oriented.

Response to Questions from

The Honorable Marilyn Lloyd Bouquard

Question 1. I noticed that you refer to the High Temperature Materials Laboratory at ORNL as an important new major facility. I also notice that it is not included in the President's Budget. Do you think the HTML is still an important facility for the future, and do you think it should be included in the FY 1983 Authorization and Appropriation? Are you doing high temperature materials research now, and what kind of results are you getting?

It is my belief that the High Temperature Materials Laboratory is an extremely important new facility and that it should be included in the FY-83 Authorization and Appropriation. Energy development is closely connected with materials. Nearly every technology - nuclear fusion, breeders, advanced coal-burning plants, coal liquefaction technologies - is limited by some deficiency in materials properties at high temperature. Problems arise due to corrosion, as witnessed by the many difficulties experienced in high temperature heat exchangers. There are problems of mechanical strength at elevated temperatures, resistance to radiation damage, and sometimes a limited life of an alloy or a ceramic. But beyond that, energy efficiency increases at elevated temperature and new developments such as automotive turbines or higher temperature process heat sources are limited by the lack of suitable materials. Finally, high temperature alloys now in common use are very demanding of materials which are in short supply, many of which are not available from mineral resources in the U. S. Materials such as chromium, cobalt, platinum, and tungsten are essential for current high temperature alloys, and we must often obtain these materials from countries that are not always reliable suppliers. New alloys and new ceramics can be developed which will be much less demanding of such exotic materials.

The Oak Ridge National Laboratory is thoroughly convinced of the need for high temperature materials research. We have in fact put in place a high temperature materials research program even though the High Temperature Materials Laboratory was not funded in FY-83. We are greatly hampered by the lack of suitable facilities, but we nevertheless feel so keenly about the importance of high temperature materials research that we are doing everything we can to put together a strong multidisciplinary program in this area of research. Our efforts are not unrewarded. Just in the last few weeks one of our metallurgists, Dr. C. T. Liu, has developed a new material composed of nickel and aluminum which is two times stronger than stainless steel at room temperature and five times as strong at elevated temperatures around 800°C. It is stronger than super-alloys by about a factor of two at elevated temperatures, it corrodes more slowly than stainless steel, and it contains no chromium or other strategic materials which are such a large fraction of stainless steel and super-alloys. We are also making good progress on ceramics for high temperature service. In particular, we are finding that mixing trace amounts of metals with ceramics enhances their mechanical strength very substantially.

It is my firm belief that advances in high temperature materials science will result in an astonishing array of new materials of great importance to the energy development in the United States. A laboratory devoted to this research could do much to speed up the process.

Question 2. You have a very large Basic Energy Sciences Program at ORNL. Could you give the Committee one or two examples of something interesting that is happening there just now?

It is indeed a pleasure to report that the Basic Energy Sciences Program at ORNL is thriving. Many good things are happening. One of them, in fact, is the nickel aluminum intermetallic compound I described briefly in my answer to the first question. Some other particularly interesting items are:

(1) Chemists are developing a new method for determining the details of the structure of coal whereby we can now ascertain whether the carbon atom in coal is connected to a hydrogen atom, an oxygen atom, or another carbon. Our ability to do this is expected to unlock the many secrets of very complicated materials such as coal.

(2) A problem in achieving fusion energy is the degradation of magnet coil windings by radiation damage. While our recent findings show favorable properties of new polyimide plastics as electrical insulators after irradiation, they also show serious problems arising in the copper protective windings which, in some designs, would degrade seriously due to radiation damage.

(3) Another interesting "breakthrough" in our materials program is the development of laser annealing techniques. By using the Cornell Synchrotron Light Source (we are users too!), a technique was devised for studying very

rapid melting/solidification processes on a time scale of a few billionths of a second. This development opens up highly interesting vistas in melting phenomena, crystal growth, and other materials properties.

(4) New experiments by ORNL scientists using photoelectron spectroscopy are unraveling important questions in the theory of gas molecules (how atoms join together to form molecules). Results from this research can be used advantageously in making clearer the molecular reactions important in such practical areas as catalysis and combustion.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139

May 3, 1982

The Honorable Don Fuqua
Suite 2321
Rayburn House Office Building
Washington, DC 20515

Dear Congressman Fuqua:

I enclose a response to the questions you forwarded to me in your letter of March 31, 1982.

Thank you once more for the opportunity to present my views regarding nuclear science to your Committee.

Sincerely yours,

Herman Feshbach

Herman Feshbach
Chairman
DOE/NSF Nuclear Science
Advisory Committee

HF*eac
Enclosure

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COMMITTEE ON SCIENCE
AND TECHNOLOGY

ANSWERS TO QUESTIONS RAISED BY MEMBERS
OF
THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
OF
THE U.S. HOUSE OF REPRESENTATIVES

Professor Herman Feshbach
March 25, 1982

1. The FY 1983 DOE request for nuclear physics has been reduced below FY 1982 funding levels. The justification for this reduction is unclear.

   In my testimony I expressed the opinion that the reduction in the FY 1983 DOE request for nuclear physics is unwarranted in view of the existing scientific opportunities of the field and catastropic in view of its impact on the capability of U.S. nuclear science. In the past, nuclear science has made essential contributions to national security, to industry -- particularly to the energy sector, to medicine, and to other sciences and technologies. The need of these areas for informed understanding of nuclear processes and for manpower trained by nuclear scientists is if anything greater than it has been in the past. The FY 1983 DOE request, unless remedied, would take a large step toward rendering U.S. nuclear scientists unable to continue to make significant advances of a fundamental nature or to provide the scientific basis and trained personnel for applying these to the problems connected with the most important national concerns.

2. Did the Nuclear Science Advisory Committee which you chair have any input for DOE's FY 1983 priorities, especially for nuclear physics facilities?

   The Nuclear Science Advisory Committee has given its priorities in the December 1979 Long Range Plan. It is at the present revising the section of that report which deals with the scientific opportunities. Special Subcommittees dealing with Instrumentation, Heavy Ion Physics, the Electromagnetic Probes of Nuclei, Computer Needs, and Manpower have reported or are about to report and their recommendations transmitted to the DOE and NSF. In addition, each year the Facility Subcommittee evaluates and recommends construction proposals. Unfortunately, the DOE FY 1983 budget proposal is so constrained that these recommendations were not implemented, and no new construction is in that budget. As emphasized in the testimony, the present construction rate is much too small, in the long run, to maintain the U.S. capability in nuclear science.

   At the November 1981 meeting the FY '82 and possible FY '83 budgets were discussed. The Committee responded with a brief report in which the budget level as well as the actions contemplated by the agencies in response to these budgets were protested. A set of criteria which any plan to implement the budget reductions should satisfy were formulated as well. It is my impression that these were in part adopted by the funding agencies.

3. What is the status of Brookhaven's proposal to upgrade the Double Tandem Accelerator Facility?

   In the recent meeting of NSAC this proposal was given a positive rating by the Committee as cost-effective and filling a need in the heavy ion research program which had been stressed in a report of a special NSAC Subcommittee on Heavy Ions.

4. The Brookhaven Double Tandem has a large demand from users and has great potential for upgrade in the near future. Is the DOE's proposal to initiate phase-out of this facility in 1983 justified?

As the testimony emphasized, one of the most, if not the most, unfortunate consequence of the budget request would be the initiation of the phase-out of the Brookhaven Double Tandem Facility. This is one of the most powerful facilities of its kind, and very recently has been substantially improved. Moreover, it has served the nuclear community well and has been involved in the education of several generations of graduate students. Other strategies, in which it would not be phased out, are possible, but these would generate another set of problems.

5. Are the DOE and NSF nuclear physics program complementary? Is some overlap desirable? What do you see as the future roles of NSF and DOE respectively in nuclear physics research?

Among the major strengths of the support structure of American science one must list its flexibility, its ability to respond to challenges, the absence of heirarchal constraints, the mobility of the research scientists and importantly its close integration with education. Adequate support is essential to maintain these features. Not the least of the unfortunate consequences of the inadequate funding of nuclear physics has been the erosion of these essential elements. Of great importance as well, have been the pluralistic sources of funding. Each agency will have a somewhat different agenda with differing emphases. A project that doesn't fit one may well fit the other, contributing significantly to the flexibility of the system. The DOE clearly has responsibility for the national laboratories. It also supports several university accelerators and user groups. The NSF funds a greater number of university-based accelerators but also supports major laboratories at Indiana University and Michigan State University. The superconducting linear accelerator developed at the Argonne National Laboratory will be exploited by Florida State University, an NSF client, while the superconducting configuration of Michigan State University forms the basis for upgrading the accelerators at Oak Ridge National Laboratory and Texas A & M. The cooler and storage ring proposed by Indiana University has already had a significant impact on accelerator design nationally. It is very likely that only these various imaginative options would not have been explored if there were only one funding agency. Because of the strong beneficial interaction between education and research, the reduced participation of universities in nuclear physics which has attenuated so sharply in the last decade must be reversed. Both the NSF and DOE should respond to this need.

6. Is the shutdown of some university-based facilities justified?

As mentioned at the end of the answer to the preceding question, one of our major concerns has been the reduced participation of universities in nuclear research. This has been a consequence, in part, of the reduction in the number of university-based accelerators by an order of magnitude in the last decade. It is my belief that this process should go no further; that those university accelerators which become obsolete should be upgraded or replaced rather than terminated. It is of great importance to encourage the synergism between research and education, not only in behalf of the quality of the research, but also for the future of the field.

7. What are the prospects for greater international cooperation in nuclear physics facility construction and operation?

In my opinion such international cooperation is essential. This is particularly true for the very large facilities on the horizon such as the "kaonfactory" and "Venus", a proposal for a relativistic heavy ion facility. International cooperation should include the sharing of facilities. Some such cooperation is now going on. It should be more widespread. At the present time this cooperation is arranged between the laboratories directly but it would be useful if explicit official recognition by the agencies could be made and if inter-government channels could be formed so as to expedite such cooperation. Multi-national arrangements may be needed in some cases.

FISCAL YEAR 1983 DEPARTMENT OF ENERGY BUDGET REVIEW

(Fossil Energy and Basic Research)

TUESDAY, MARCH 30, 1982

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
Washington, D.C.

The subcommittee met, pursuant to recess, at 9:35 a.m. in room 2318, Rayburn House Office Building, Hon. Harold L. Volkmer presiding.

Present: Representatives Volkmer, Sensenbrenner, Gregg, Carney, and Dunn.

Staff present: Martha Krebs, staff director; and William T. Harvey, minority counsel.

Mr. **Volkmer**. The subcommittee will come to order. Today we will conclude our series of hearings on the review of the fiscal year 1983 DOE budget request. In this hearing we shall hear from Dr. Alvin Trivelpiece, Director of DOE's Office of Energy Research, and his associates, Dr. Richard Kropschot and Dr. James Leiss. They will present the Department's proposed fiscal year 1983 basic research program, basic energy sciences, high energy, nuclear physics, and related activities for which the administration requested over $850 million.

At this time, I would like to have the opening statement of our chairman, the Honorable Don Fuqua from Florida, made a part of the record. He is not able to be here at this time.

Mr. **Volkmer**. Before we begin, Mr. Carney, do you have an opening statement which you wish to make at this time?

Mr. **Carney**. I would appreciate it, Mr. Chairman.

Mr. **Volkmer**. All right. Proceed.

STATEMENT OF WILLIAM CARNEY, A MEMBER OF CONGRESS FROM THE STATE OF NEW YORK

Mr. **Carney**. Thank you, Mr. Chairman, for the opportunity to make a statement on a subject for which I have very strong regard.

These programs focus on the fundamental laws of nature relating to matter and energy. The scientific leadership, which this country has maintained for the past 50 years, is attributable to the attention we have given to the basic research programs. However, we are not allowed to rest on our laurels.

This maintenance of leadership requires continual intelligent use of present facilities and the design, construction, and operation of newer, more powerful instruments for future experimentation.

The Brookhaven National Laboratory is the site selected by the high-energy physics community to construct the exploratory facilities for the next decade. The progress made on this project to date is very impressive, and the High Energy Physics Advisory Panel has concluded that recent successes in the development of that program at Brookhaven have greatly increased technical confidence in the facility design.

Therefore, the major obstacle at this time appears to be the prospect for adequate funding. The funding problem is both immediate and in the future.

We were very disturbed to see that no construction funds were made available in the fiscal year 1983 budget for the Isabelle project. We will make every attempt to alleviate that problem. However, we also feel very strongly that there must be a long-term commitment by DOE to complete the design and continue construction of this facility to its completion in the latter part of the decade. I hope that we can get the commitment.

We discussed this aspect with Secretary Edwards during his appearance before the committee on Thursday, March 4, 1982. A review of that testimony indicates that Secretary Edwards' only problem seems to be with the design of the superconducting magnets.

We are, therefore, very pleased that since those problems are resolved, we should be able to go forward in an expeditious manner to complete the project.

I thank you, Mr. Chairman, for the opportunity to make that opening statement.

Mr. VOLKMER. I thank the gentleman.

You may now proceed, Dr. Trivelpiece. Your prepared statement will be made a part of the record, and it will be included in the record in its entirety. If you wish to summarize, you may do so. If you wish to review the statement or read the statement, you may do so. You may do however you see fit.

STATEMENT OF ALVIN W. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY, ACCOMPANIED BY RICHARD H. KROPSCHOT, ASSOCIATE DIRECTOR, OFFICE OF BASIC ENERGY SCIENCES/OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY; JAMES E. LEISS, ASSOCIATE DIRECTOR, OFFICE OF HIGH ENERGY AND NUCLEAR PHYSICS/OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY; AND D. D. MAYHEW, ACTING DIRECTOR, FINANCIAL MANAGEMENT DIVISION, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. TRIVELPIECE. Thank you, Mr. Chairman. You have already introduced two of the people who are here with me. In addition to Dr. Kropschot and Dr. Leiss, I would like to introduce Ms. Toni Joseph who is the Director of the Field Operation Management part of my activities.

As you indicated, I would like to have my prepared statement submitted for the record. What I am going to do is give a brief overview, and then I will turn to Drs. Kropschot and Leiss and ask them to describe some of the activities in their programs. After that we would be happy to answer any questions including those questions which were submitted earlier by Mr. Fuqua. Although those questions are somewhat detailed in character, we would be glad to supply those answers for the record. If there are any aspects of them that you would like to have discussed today, we would be happy to do so.

[The information follows:]

1. Mr. Fuqua: The FY 1983 request for basic energy science is 16% greater than the FY 1982 appropriation. This increase is justified, in part, by additional operating costs for new research facilities coming on-line in FY 1983 and by increased operating costs for existing facilities. Given the increasing use of large facilities, is improved long-range facilities planning required in the basic energy sciences, similar to that already adopted by the high energy and nuclear physics programs?

Dr. Trivelpiece: In view of the 10 year lead time between when a large research facility is first conceived and it becomes available for use, our long-range facility planning process for basic energy sciences is in need of improvement. However I'm not convinced that the model for planning used by the high energy and nuclear physics communities is appropriate for the basic energy sciences. The high energy and nuclear physicists are relatively small, internally well coupled, coherent groups that receive almost their total support from the DOE and NSF. Even so they require separate advisory or planning committees.

Basic Energy Sciences, on the other hand, is composed of several large and a few small distinct disciplines having rather diverse interests. Further these disciplines are supported by a multiplicity of sources including the private sector, with DOE being a minor participant in some cases -- atomic physics and engineering as examples. It is difficult to imagine a single High Energy Physics Advisory Panel type committee projecting the future needs of this broad spectrum of sciences, weighing their needs and establishing a prioritized list for the DOE to consider either independently of or in concert with other agencies. What we are talking about is a central planning and analysis mechanism that goes beyond the DOE and considers the needs of the Nation as a whole. Such a mechanism is not entirely consistent with our pluralistic support of science in the United States and therefore requires careful thought in its establishment.

In BES we are seeking to improve our long-range facilities planning process and are in discussion with several groups both internal and external to the National Academy of Sciences. It is an evolutionary process in which we are both active and passive. By that I mean we are both active in seeking advice to resolve issues such as needs for advanced neutron facilities and passive in responding to needs generated by the scientists themselves such as the Combustion Research Facility. We actively participate in and support various committees of the National Academy of Sciences that address the needs of specific disciplines such as chemistry and those that consider multidisciplinary fields such as condensed matter science.

Establishing priorities of needs is only part of the overall problem as we are seeing in the current set of new facilities just coming on stream. The Combustion Research Facility - Sandia, Livermore - the Intense Pulsed Neutron Source at Argonne National Laboratory, and the National Synchrotron Light Source at Brookhaven were all authorized at a time when basic science budgets were expanding. Since that time this expansion has been curtailed and now the operational build-up of these facilities imposes severe strains on our budgets. We may be forced to prematurely terminate operation of the Intense Pulsed Neutron Source in order to sustain operation of other neutron facilities. Plans for other new facilities such as the High Temperature Materials Laboratory at the Oak Ridge National Laboratory have been deferred as have our plans for a Chemistry and Materials Research Laboratory at LBL. Other new facilities or plans for expansion of existing facilities are waiting in the wings. What I am pointing out is that plans, no matter how well developed, based on scientific criteria are subject to drastic revision by conditions completely devoid of technical consideration. In this sense, then, long-range planning is a highly dynamic process that involves political and economic processes as well as scientific value judgments. In this context the Congress and committees such as yours are as strong if not stronger partners in the planning and follow-thru processes.

Within our own restricted sphere of activities and interests we are becoming increasingly aware of the need for improved facility planning and have this management requirement near the top of our priority list. We have already strarted our planning activities and welcome the interest of this Committee.

2. Mr. Fuqua: The operating and/or construction responsibilities for several facilities have been transferred between the Department of Energy (DOE) and the National Science Foundation (NSF); for example, the Stanford Synchrotron Radiation Laboratory (SSRL) from NSF to DOE, and the Michigan State University Superconducting Cyclotron from DOE to NSF. Are these transfers indicative of some ambiguity over the respective roles of DOE and NSF in areas of mutual research interest?

Dr. Trivelpiece: These transfers are intended to optimize Federal management of both the Stanford Synchrotron Radiation Laboratory project and the National Superconducting Cyclotron Laboratory project and do not reflect any ambiguity over the respective roles of DOE and NSF.

Since the Michigan State Cyclotron Laboratory's inception in the early 1960's, NSF has been the lead Federal agency responsible for that Laboratory. NSF has supported and managed fabrication for the older K-500 superconducting cyclotron which will serve as injector to the larger K-800 superconducting cyclotron being built under the National Superconducting Cyclotron Laboratory project. NSF provides for operations and in-house research at the existing Cyclotron Laboratory.

DOE assumed responsibility for the National Superconducting Cyclotron Laboratory construction project, following OMB's request in the fall of 1978 to do so. Prior to that, AEC/ERDA/DOE involvement had been limited to support of a Michigan State nuclear chemistry group which conducted its research as part of the overall Cyclotron Laboratory effort. Indeed, in the early 1970's, NSF and AEC/ERDA agreed to transfer of this nuclear chemistry group from AEC/ERDA to NSF in order that management of in-house Cyclotron Laboratory research be consolidated under a single Federal agency. Our understanding with NSF and OMB had been that, upon completion of the National Superconducting Cyclotron Laboratory project, NSF would be responsible for management and support of the entire Michigan State Cyclotron Laboratory including components provided under the DOE-managed construction project.

DOE and NSF have been able to reach a viable understanding under which DOE managed the line item construction project while NSF managed operations of the on-going Cyclotron Laboratory. However, the mere presence of two agencies created some conflicts--e.g., priority for utilization of Laboratory staff. Also, the two-agency involvement has inadvertently produced situations whereby the University could seek decisions from either agency without the other's being aware of it. For better Federal management of the National Superconducting Cyclotron Laboratory construction project (including integration into the existing Cyclotron Laboratory), transfer of the project to NSF is desirable.

We are convinced that the transfer can be executed with little interference in the construction work at Michigan State.

Transfer of the Stanford Synchrotron Radiation Laboratory (SSRL) also makes good sense. Here the purpose is to bring together under one agency the support and coordination of the two large synchrotron radiation facilities in the U.S. These are SSRL and the National Synchrotron Light Source (NSLS) at Brookhaven. Construction of the latter through DOE support has just been completed.

NSF provided the major support to build, operate and upgrade the SSRL beginning in 1972. SSRL, however, totally depends on and reimburses two DOE High Energy Physics facilities, the Stanford Linear Accelerator (SLAC) and the Stanford Positron-Electron Asymmetric Ring (SPEAR). SLAC, a 2-mile-long accelerator, injects electrons into SPEAR, where they circulate at up to 4 billion electron volts and emit intense synchrotron radiation (ultraviolet and x-ray regions) from which any narrow band can be selected for a given experiment.

Many DOE- and NSF-supported researchers are taking advantage of these intense radiations for energy-related and general science. Since DOE owns and operates the NSLS, which is the other of the two largest U.S. facilities for synchrotron radiation research, and also owns and operates SLAC and SPEAR, there is considerable management effectiveness to be gained through transfer of responsibility for SSRL to DOE.

Mr. Fuqua: How might the definition of these roles be improved in order to ensure that the respective budget requests are more consistent from year to year?

Dr. Trivelpiece: The role of the DOE Basic Energy Sciences program, which includes support for NSLS and now SSRL, is to advance those scientific areas which are most likely to provide a strong base of knowledge, instrumentation, and skilled manpower in technologies critical to this Nation's energy needs. The NSF role does not and should not have this focus on the needs of energy technologies.

In order to conduct forefront experiments in basic nuclear research, materials research, and chemical research, large national facilities are needed. DOE has built and manages a number of unique facilities. Because of their size and complexity, these facilities are best sited at national laboratories or large, single purpose laboratories. The DOE program gives strong attention to construction and management of these unique, forefront facilities for advanced experiments. The NSF, on the other hand, continues to emphasize support of the smaller university-based programs that have a major educational and training component. While there are these respective emphases in the DOE and the NSF programs, there are some variations in each case. We consider some flexibility in these rules important to fostering basic research in the U.S. and view it as one of the strengths of the U.S. approach. It does work well. The overriding mangement concern is to insure that there is adequate coordination between the two agencies. Through joint advisory committees and frequent consultations between the staffs of the two agencies there is good coordination, and we would see no reason to recommend a change. We recognize the problems shifts such as this kind present and do not anticipate more of them in the near future. We do believe it is prudent to permit shifts of this kind when effective management calls for it as in this instance. We believe further, that by centralizing management of support for operation of the two major U.S. synchrotron radiation facilities, budget consistency will be assured in the future.

3. Mr. Fuqua: The FY 1983 request for nuclear physics operating expenses is below the FY 1982 appropriation. How can this reduction be justified, given the increased costs to operate existing facilities and the additional operating expenses required to operate new or improved facilities (particularly in heavy ion nuclear physics) coming on-line in FY 1983?

Dr. Trivelpiece: The FY 1983 request for Nuclear Physics operating expenses is $113.4 million. This is $1.0 million above the FY 1982 appropriation. But, when escalation and electric power rate increases are taken into account, there is a real reduction in level of activity in the Nuclear Physics program.

The Office of Energy Research is convinced of the importance of basic nuclear research in general and the Nuclear Physics program in particular. Nuclear research remains an important area of basic science. The Nuclear Physics program provides to the Department and the Nation an essential base of fundamental knowledge, advanced techniques and instrumentation, and manpower in critical technologies. However, within an overall constrained Office of Energy Research budget, difficult decisions had to be made.

Mr. Fuqua: What is the justification for the sharp decrease in heavy ion nuclear physics compared with medium energy physics?

Dr. Trivelpiece: In setting research priorities, Nuclear Physics assigned highest priority to the internationally unique scientific capabilities of the Clinton P. Anderson Meson Physics Facility (LAMPF), the William H. Bates Electron Linear Accelerator, and the Lawrence Berkeley Laboratory Bevalac. LAMPF and Bates accommodate not only medium energy nuclear physics but are the only accelerator facilities budgeted under the medium energy category. In order that these facilities operate effectively in FY 1983 in the face of rapidly escalating electric power rates, a modest increase in medium energy nuclear physics is essential.

For the record, I will submit tables describing utilization of the major Nuclear Physics accelerator facilities and electric power rates at these facilities.

Mr. Fuqua: What does this mean for operation of the facilities at LBL and BNL?

Dr. Trivelpiece: Bevalac will operate for research 2000 hours in FY 1982 as compared with 2934 hours in FY 1981. The Brookhaven Double MP Facility will operate 4560 hours for research in FY 1983 as compared with 5500 hours in FY 1982.

3. Dr. Trivelpiece: A table showing machine utilization is provided for the record.

UTILIZATION OF FACILITIES--NUCLEAR PHYSICS
(Beam hours for research)

	Actual FY 1981	Estimated FY 1982	Pres. Budget FY 1983
LAMPF	3104	3100[1]	2500[2]
Bevalac	2934	2000[3]	2000[4]
Bates	2976	2500[5]	3000
Holifield	1900	2400[6]	4000[7]
LBL 88-inch Cyclotron	4059	3500	3500
BNL Double MP Tandem	4112[8]	5500	4560

1 Partial Defense Programs support ($2.0 million) in FY 1982 for accelerator operations.
2 Assumes zero Defense Programs support in FY 1983 for accelerator operations.
3 Four-month shutdown for Uranium-Beams construction project.
4 Also restricution to 670 MeV/nucleon operation; WAPA power availability would mean running at higher energies and/or additional time.
5 Four-month shutdown for testing by vendor.
6 Three-month shutdown for testing by vendor.
7 Restriction to 1700 hours on high energy running with ORIC coupled operation.
8 Shutdown for part of FY 1981 for completion of an upgrade project.

Dr. Trivelpiece: The following table shows projected electric power rates for the major Nuclear Physics facilities.

ELECTRIC POWER RATES
(mils/kwH)

	FY77	FY78	FY79	FY80	FY81	FY82	FY83
LAMPF	17.2	20.5	23.3	24.5	30.6	35.0	41.5
Bates	36.5	35.5	37.8	52.4	62.5	68.7	82.4
LBL	30.0	38.5	32.9	42.5	59.0	64.4	72.0
Holifield/ORIC	20.0	22.7	24.4	24.5	33.6	38.9	43.0
BNL Double MP	28.3	28.4	30.2	43.5	71.3	83.8	98.5

4. Mr. Fuqua: No FY 1983 construction funds are requested for the ISABELLE high energy physics project at Brookhaven National Laboratory. If the project were to go forward, assuming that the current technical problems associated in the magnets are resolved, what is the new total estimated project cost? If the project were to be terminated, what are the termination costs and what are the total sunk costs through FY 1982?

Dr. Trivelpiece: The ISABELLE project has been placed in an R&D mode, and we have not requested any construction funds in the FY 1983 budget.

Based on the present design and a scheduled completion date of 1987, the total estimated cost of the project as reevaluated in October 1981 is $465M. This included $121M in project construction funds committed through FY 1982 and assumed a continuing construction effort in FY 1983. If the project were terminated, we estimate that the direct termination costs would be in the range of $10-20M. This is in addition to the loss of much of the $121M of construction funds and some of the $52M of R&D funds invested to date.

There is a clear need for a major new high energy physics research facility which would be completed near the end of the decade. A decision to proceed with such a future facility will be based on the success of the R&D efforts at BNL, overall high energy physics program requirements, and budget considerations. Consideration of current outyear budget projections for HEP suggest that this facility may have to be a lower cost facility than the present ISABELLE project.

5. **Mr. Fuqua:** The SSRL facility is collocated with the SPEAR accelerator at Stanford. Please provide the total costs for operating the whole facility; indicate facility operating costs and supporting research costs. Please provide a list of organizations whose researchers use SSRL.

Dr. Trivelpiece: SPEAR facility operating costs are divided by formal agreement 50:50 between SSRL and High Energy Physics (HEP), so that each allocates $1.73 million for that purpose in FY 1982. On the other hand HEP research using SPEAR is an integral part of the total SLAC research program and is not budgeted for separately, but we estimate costs for a full year's program of SPEAR related HEP research to be about $2.5 million. This does not include detector construction costs. The new Mark III detector which is just being completed cost $4.5 million. Down time during this transition is reflected in the FY 1982 HEP research cost, estimated to be only $1.8 million. The table summarizes the anticipated FY 83 expenditures.

	(Dollars in Millions)			
	Support from BES	Support from HENP	Support from Other Sources	Total
Operation of SPEAR to Provide Beams	2.1	2.1	-	4.2
Research Using Spear	-	2.5-3.0	-	2.5-3.0
Operation of SSRL Facility	4.7	-	-	4.7
Research at SSRL	*	-	2.0-4.0**	2.0
Total	6.8	4.6-5.1	2.0-4.0	over 13.4

* BES direct support of synchrotron radiation research at SSRL in FY 83 will be less than $1.0M and comes from various BES projects. It will range from major dependence on SSRL to short time exposure of a sample.

** Most research at SSRL is supported by other agencies (e.g., NSF, DARPA, NIH) or by industry. Information on the total level of Federal and industrial support is not available at this time. An estimate is at least $2M based on the number of experimental stations available and in use at present, and taking into account relevant activities at the home institution.

I am providing a list of the extensive group of institutions which used SSRL during the one-year period ending last March. The list is typical of the current year as well. These scientists are supported by grants and contracts from the National Science Foundation, DOE and other agencies, as well as by private funds of their home organizations. The table is a DOE/BES estimate of these costs.

SSRL USER INSTITUTIONS

As of March 31, 1981 experimenters from the 122 institutions in this list were officially involved with active proposals at SSRL. Of these institutions 26 are from 11 different foreign countries. The 96 United States institutions include 54 universities, 31 private corporations and 11 government laboratories.

Universities

Albert Einstein College of Medicine, Amherst College, Boston University, California Institute of Technology, California State University-San Jose, Carnegie Institute of Washington, Case Western Reserve University, Center For Astrophysics, Center for Energy and Environmental Research, Children's Hospital Medical Center, Colorado State University, Columbia University, Dartmouth Medical School, Duke University, Georgia Institute of Technology, Georgia State University, Iowa State University, Johns Hopkins University, Massachusetts Institute of Technology, Michigan State University, New York University, North Carolina State University, Princeton University, Purdue University, Rice University, Stanford University, State University of New York-Buffalo, State University of New York-Stony Brook, Texas Technical University, University of California-Berkeley, University of California-San Francisco, University of California-Santa Barbara, University of Chicago, University of Cincinnati, University of Connecticut, University of Delaware, University of Florida, University of Hawaii, University of Houston, University of Illinois, University of Michigan, University of Minnesota, University of Nebraska, University of North Carolina, University of Oregon, University of Pennsylvania, University of Pittsburgh, University of Texas-Austin, University of Virginia, University of West Virginia, University of Washington, University of Wisconsin, Washington State University, Wayne State University.

Private Corporations

Bell Laboratories, Boeing Company, Catalytica, Inc., Celanese Research Company, Charles F. Kettering Research Laboratory, Chevron Research Company, DuPont, Inc., Eastman Kodak Company, Exxon Research & Engineering, Ford Motor Company, General Electric Research & Development Center, General Motors Research Laboratories, Gulf Research & Development, Hewlett Packard Laboratories, Hughes Aircraft Company, IBM Research Laboratories-San Jose, IBM Research Laboratories-Watson Research Center, Institute for Cancer Research-Philadelphia, Lockheed Research Laboratories, Monsanto, Rockwell International, Shell Development Company, SRI International, Tribo-Materials Development Corporation, U.S. Steel, Western Electric, Westinghouse Research and Development, Xerox-Palo Alto Research Center, Xerox-Webster Research Center.

Government Laboratories

Argonne National Laboratories, Brookhaven National Laboratory, Lawrence Livermore Laboratory, Los Alamos National Laboratory, Michelson Laboratory-China Lake, National Bureau of Standards, National Institutes of Health, Naval Research Laboratory, Oak Ridge National Laboratory, Pittsburgh Energy Technology Center, Sandia Laboratories, Stanford Synchrotron Radiation Laboratory, Lawrence Berkeley Laboratory.

Foreign Institutions

Azerbaijan State University, BESSY, Chalmers University, KFA-Julich, Laboratory of Molecular Biology, Linkoping University, L.U.R.E., McMaster University, National Institute for Researches in Inorganic Materials, Photon Factory, Saitama Institute of Technology, Simon Fraser University, Universitat-Bern, University of Geneva, University of Groningen, University of Leicester, University of Manchester, University of Reading, University of Sheffield, University of Sydney, University of Technology, University of Tel Aviv, University of Tokyo, University of Toronto, University of Rennes, Yamanashi University.

6. Mr. Fuqua: The HEPAP Subpanel report recommends undertaking an electron-positron experiment called the Stanford Linear Collider (SLC), which would require construction funds in FY 1984. Please describe this project; provide a statement of need and its projected budget. How much of the SLAC R&D budget in FY 1982 and FY 1983 will be provided for this project; please detail operating expenses and capital equipment requirements. What percentage of SLAC's operating budget do these funds represent.

Dr. Trivelpiece: The SLAC Linear Collider (SLC) is an accelerator research project which is central to the long term development of a very high energy electron-positron collider, and at the same time offers the opportunity to do some very exciting physics in a timely fashion. In accelerator research, SLC will pioneer a new technology which holds high promise for significantly reducing costs of future machines in the energy regime of 300 GeV on 300 GeV electron-positron collisions and above, thus making accessible interaction energies not otherwise economically feasible. The SLC is an example of the relatively large R&D projects which are now necessary in the development of new accelerator systems. In physics research, the SLC will allow U.S. physicists to participate in tests of the recently developed "Gauge Theory" which interprets both the weak nuclear force and the electromagnetic force as different manisfestations of one basic force. This theory predicts the existence of heavy particles, the Z^0 , W^+ and W^- , which should be produced copiously when the center-of-mass energy of the electron-positron collisions in SLC matches the mass of the particles. The decay of these particles into quarks, leptons and perhaps other more exotic particles will give a vast amount of information about a broad range of basic physics questions ranging from the number of elementary building blocks which make up our world, to the possible relation of the strong interaction to the weak and electromagnetic, and to the conditions in the early universe when it was only a few seconds old.

The SLC will use the existing SLAC two mile linear accelerator, upgraded to a final energy of 50 GeV (SLED II), to simultaneously accelerate an intense electron bunch and an intense positron bunch on each cycle. At the end of the linac a splitter diverts the electron and positron bunches in opposite directions into two halves of a ring of magnets. The electron and positron bunches travel around their respective half rings until they come to a collision area where a collection of special lenses focuses each of the particle beams to very tiny spots, 1 to 2 millionths of a meter in diameter at the collision point where the beams meet head-on. Most of the electrons and positrons pass through the collision point without interaction and are disposed of and not used again. A fraction of the electrons and positrons will interact in matter-antimatter annihilation which is the subject of the research investigations.

In addition to the acceleration of the electron and positron bunches, the linac will also be used as part of the scheme for producing the positron bunches to be accelerated. To do this a second intense bunch of electrons is accelerated some distance behind the other two bunches. This second group of electrons is deflected out of the linac onto a positron production target. Positron beams from the target are transported back to the injector end of the linac, accelerated, damped radially, stored, compressed longitudinally and injected into the linac for final acceleration. The entire cycle repeats 180 times a second.

DOE anticpates that SLAC will submit a construction request for SLC in the near future. A detailed and evaluated budget is not available at this time, but it is anticipated to be about $100 million with a construction time of about 3 years.

In fiscal years 1982 and 1983, R&D to develop the design of SLC is budgeted at approximately $6 million and $11 million, respectively. Capital equipment costs related to the R&D effort are approximately $1.4 million in fiscal year 1982 and $600 thousand in fiscal year 1983. The total SLAC operating budget for these years is $60,500,000 and $76,000,000, respectively. The R&D for SLC is thus 10 percent in fiscal year 1982 and 14 percent in fiscal year 1983 of the total operating budget for SLAC in those years.

7. Mr. Fuqua: The University support budget has been reduced sharply in FY 1983. Will the number of universities supported be reduced or the number of people at each school? Provide the universities and numbers of people to be effected by these reductions.

Dr. Trivelpiece: The budget request for FY 1983 will support the continuation of faculty and student research participation projects at the National Laboratories (the University Laboratory Cooperative Program) and the fabrication of fuel for university reactors (the University Reactor Fuel Assistance Program). No funds are requested to continue other University Research Support (URS) program activities. Therefore, the effect on university performers (in terms of direct on-campus grants and contracts) is as follows: In the Energy Graduate Traineeship Program 57 graduate traineeships were funded in FY 1982 at 25 universities. Each traineeship was supported for two full years, 1982 and 1983. No additional support is required for these traineeships in FY 1983. In the Energy Education and Training Program, 53 universities were supported in FY 1982 for summer teacher training institutes on energy for secondary school science and mathematics teachers. Each institute supported an average of 35 participants with one university faculty member as project director. No funds are requested in FY 1983 to continue these projects. Finally, in the University Institutional Research Program three universities received funds in FY 1982 for campus-wide exploratory research programs. Each program supported an average of 10 faculty members and 15 graduate students pursuing small "proof of concept" energy research projects. This program was phased out in FY 1982 and no funding has been requested in FY 1983.

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4. No FY 1983 construction funds are requested for the ISABELLE high energy physics project at Brookhaven National Laboratory. If the project were to go forward, assuming that the current technical problems associated in the magnets are resolved, what is the new total estimated project cost? If the project were to be terminated, what are the termination costs and what are the total sunk costs through FY 1982?

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As you indicated, this is the discussion of the general sciences appropriation which includes the high energy physics program which is $429 million and the nuclear physics program which is $131 million in fiscal year 1983. Those two programs total the $562 million when the $2 million program direction request is taken into account.

The basic energy sciences program request, which is part of the energy supply research and development appropriation, is $282.9 million for fiscal year 1983. There are a collection of other programs including university research support at $5.1 million, energy research analysis at $2.6 million, advisory and oversight program direction at $3.2 million, general purpose facilities at $34.0 million, and policy and management at $0.4 million, totaling $45.3 million, making that total appropriation $328.2 million in fiscal year 1983.

Last week this committee heard from some outside witnesses, Drs. Drell, Feshbach, and Zucker. I think they have gone over much of the introductory ground that I might have otherwise covered with regard to some of these programs. Therefore, I am only going to make a few brief remarks concerning them.

The basic energy sciences program is a broad-based collection of activities that provides the basic research and technology base activities that support many of DOE's mission-oriented energy programs. High energy physics is the program that essentially seeks to understand the fundamental constituents of matter and how they interact. The nuclear physics program seeks to understand how the protons and neutrons of the nucleus of an atom organize themselves into the various forms of matter.

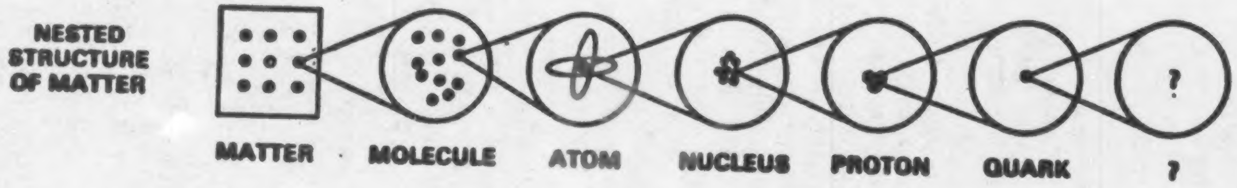
Now, each of these programs that are going to be discussed involve major facilities that are operated by the Department at several national laboratories and universities for use by scientists throughout the United States and throughout the world. In the time available this morning, it is not possible to cover the details in each of these programs, so we are going to discuss just a few of the more prominent facilities and the recently commissioned facilities and the activities that relate to them.

In my opinion these programs include activities that are the watersheds from which many important areas of U.S. science and technology and medicine have come. I believe that they are valuable in their own right in terms of the contribution to an improved understanding of the world in which we live, but I also believe that they have more than paid their own way.

Applications of nuclear medicine which have flowed from these programs are a good example. When you go to a hospital, there is a 1-in-2 chance that a nuclear medicine application will be applied to you. That amounts to a multimillion-dollar industry, and that generates taxes. These taxes, in turn, I believe, have more than paid for the research investment that has been made, not to mention the fact that this has resulted in tremendous saving of human lives and reduction in suffering.

I know that you have seen some of this before, but I would like to use the first Vu-graph (slide 1) to orient you with respect to the programs that are going to be discussed by Drs. Kropschot and Leiss.

BASIC CONSTITUENTS OF MATTER


NESTED
STRUCTURE
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MATTER
MOLECULE
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PROTON
QUARK
?

SLIDE 1

Dr. TRIVELPIECE. To the left is a symbolic illustration of matter. As you know, matter is made up of molecules, and these molecules are in turn made up of atoms. Inside the atoms are the nuclei which are made up of protons and neutrons. The protons themselves are made up of other objects which are currently called quarks. The question as to whether or not the quarks contain additional structure is an open question at the moment.

The left-hand part of the chart, the matter, molecules, and atoms, essentially describes the business of the basic energy sciences program. This program is concerned with the various states of matter, solids, liquids, and gases; and with the sciences of physics and chemistry and so on. In other words, these areas analyze how atoms relate to one another to form compounds and how those compounds exist in various states that might be useful.

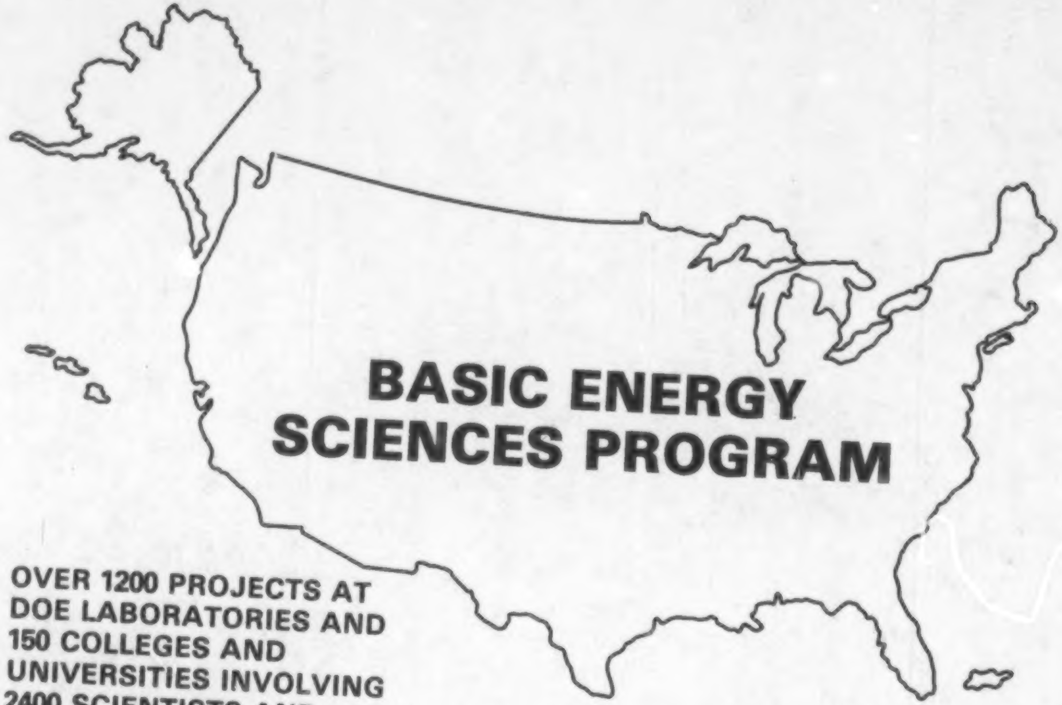
The part of the chart to the right, those figures that indicate the nucleus, proton, and quark, is the business of the nuclear physics and high-energy physics programs. These two programs are concerned with how the particles inside the nucleus relate to one another to form the various familiar states of matter, and how, within those, the structure of the protons themselves, the fundamental building blocks of nature, interact with one another.

The part of the chart to the right, therefore, is the high energy and nuclear physics program. The three items to the left are the basic energy sciences program.

With that, I would then like to turn to Dr. Kropschot who will discuss some of the activities in the basic energy sciences program.

Dr. KROPSCHOT. Mr. Chairman and members of the subcommittee, thank you for the opportunity to discuss the budget of the basic energy sciences program for fiscal 1983.

The research in basic energy sciences in the physical, biological, mathematical, and engineering sciences is conducted by engineers and scientists at the National Laboratories and over 150 colleges and universities. We conduct a program which is aimed at developing the knowledge and understanding relevant to all energy production technologies and conservation. These programs were shown in the previous slide used by Dr. Trivelpiece (slide 2).



BASIC ENERGY SCIENCES PROGRAM

OVER 1200 PROJECTS AT
DOE LABORATORIES AND
150 COLLEGES AND
UNIVERSITIES INVOLVING
2400 SCIENTISTS AND
ENGINEERS

SLIDE 2

Dr. KROPSCHOT. Our objectives of the BES program are to promote the scientific and technological insight; to establish special research facilities such as the national synchrotron light source and the combustion research facility; and to enhance U.S. scientific leadership.

The over 1,200 projects conducted by some 2,400 scientists and engineers are indicative of the magnitude of the program and offers some perspective of its broad base nature.

The objectives that we have set for ourselves to gain scientific and technological insight and to establish specialized research facilities represent just an overview of the types of programs that we conduct, but these brief objectives do not convey the magnitude of the BES program. I think it can best be seen by letting me show you some examples of accomplishments that we have achieved over the last year.

While we cannot predict the scientific long-term impact of most basic research, experience in the past decade makes us confident that the culminating effects do have significant output, and we can expect achievements from the ongoing research.

What I want to describe to you are programs that are coming out of the pipeline today, the impact of the research that has taken place over the last few years.

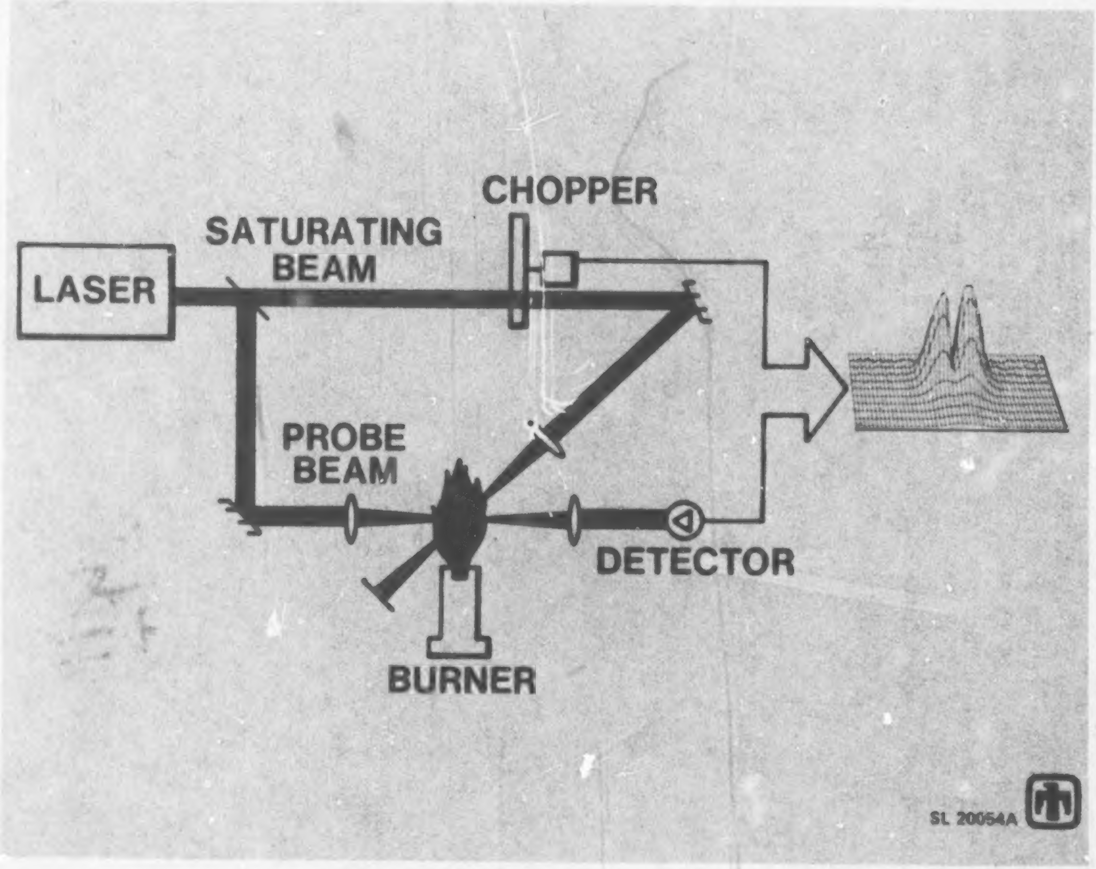
One of the accomplishments that came about just about this time last year was the completion and dedication of the combustion research facility.

Next slide, please (slide 3).

SLIDE 3

Dr. **Kropschot**. The combustion research facility at Sandia Laboratories in Livermore, Calif., has been completed. The technical program has been fully implemented. In addition to the research going on by the Sandia staff, we have a high degree of interaction between the laboratory staff and industrial and university people. Over 65 percent of the program is conducted by or in collaboration with outside users.

Next slide, please (slide 4).



LASER

SATURATING BEAM

CHOPPER

PROBE BEAM

BURNER

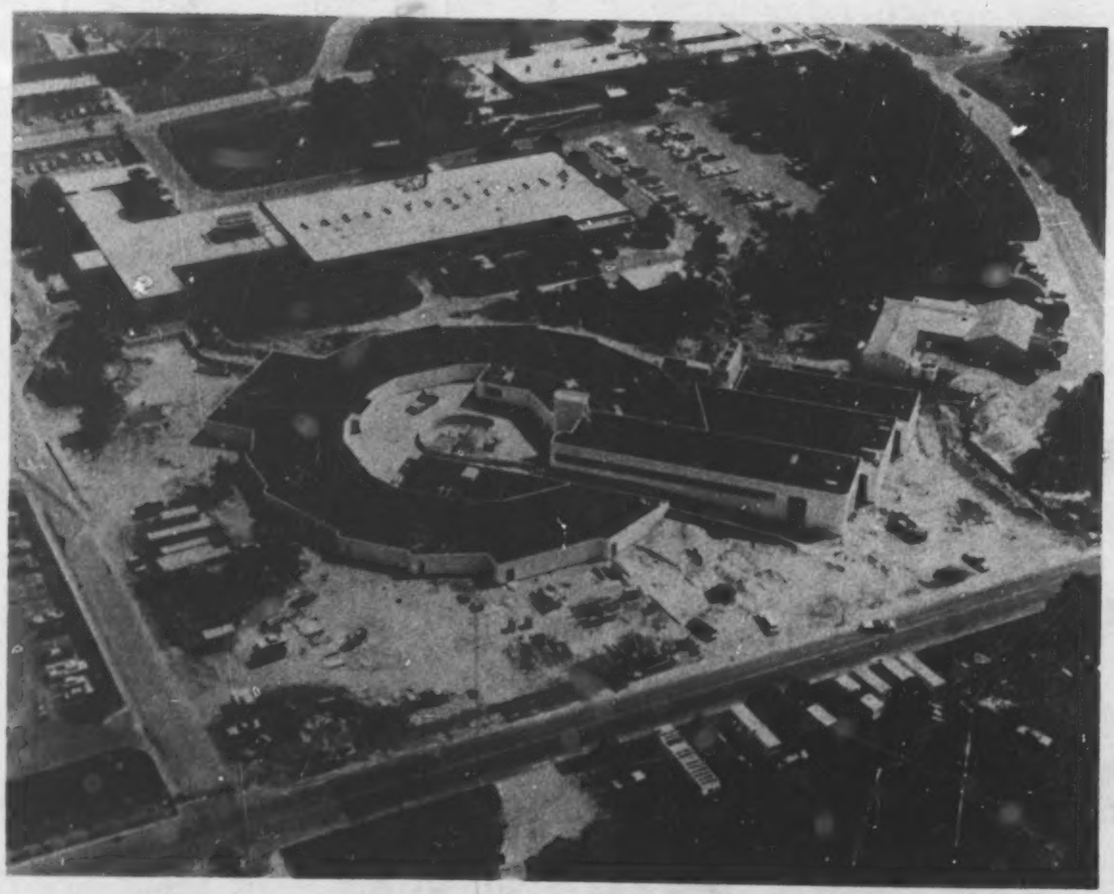
DETECTOR

SL 20054A

SLIDE 4

Dr. Kropschot. One of the accomplishments of the Sandia program in this last year is the use of laser beams and the technique developed to study trace elements or trace species in flames. Laser beams provide an excellent diagnostic tool for probing the properties of the flame. In this case, we illustrate a way that has been developed to look for minute impurities and instantaneous combustion products within the flame. That same technique can be used to measure the temperature in the flame on a point-by-point basis and measure the velocity of the particles in that flame to map out the turbulence which can lead to energy systems with higher efficiency, and lower pollution by optimization of the flame process.

Next slide, please (slide 5).



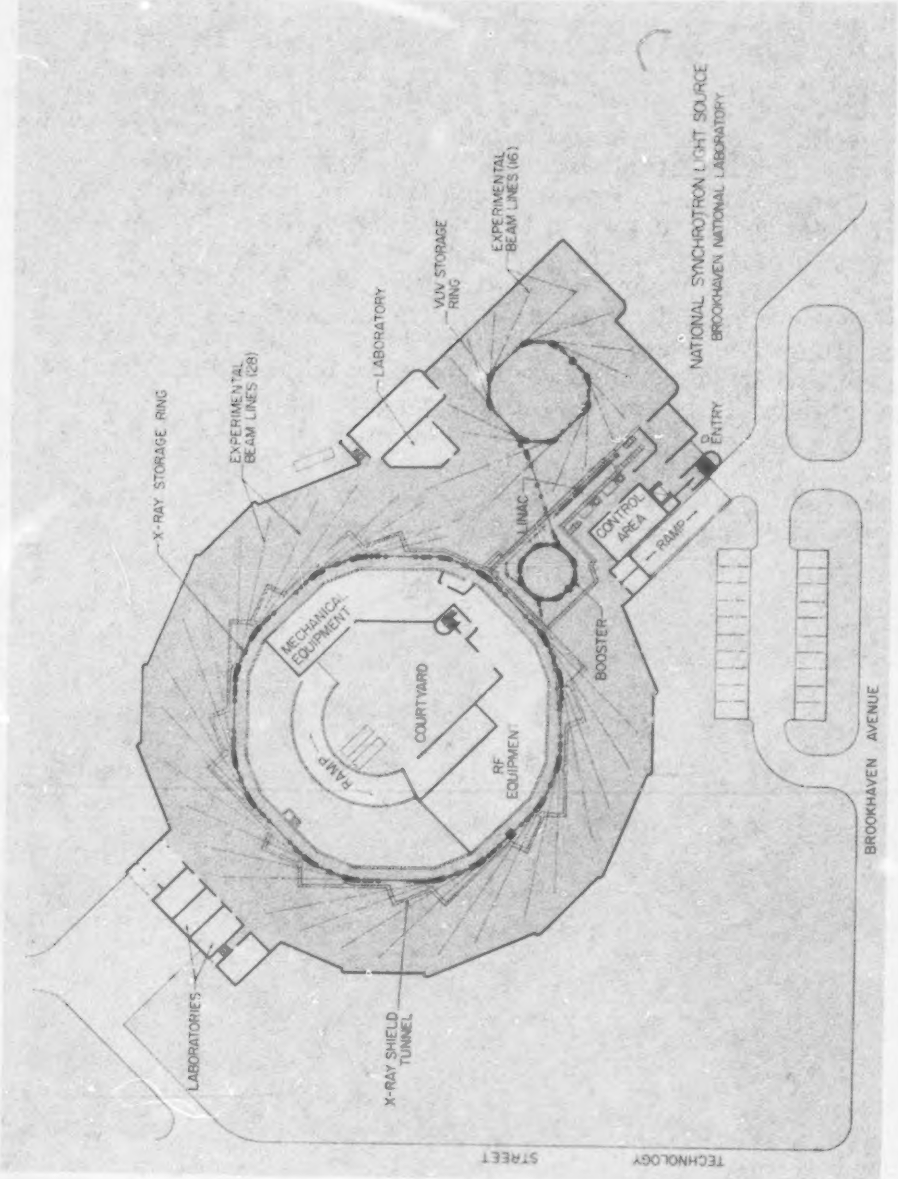
SLIDE 5

Dr. **Kropschot**. I would like to turn to the next accomplishment. Within the last 2 months, we have completed the national synchrotron light source at the Brookhaven National Laboratory, and we are in the process of implementing that program.

The visual that I have shows a unique facility duplicated nowhere in the world where the scientists and engineers can use not only the radiation that comes out of the synchrotron light source—the X-rays—but also the neutrons that are produced in the high flux beam reactor shown in the background. It is a facility that we expect will attract a significant amount of attention.

Like the combustion research facility, the national synchrotron light source has attracted a significant amount of industrial participation. There are a large number of industrial organizations which are spending their own money to build beam lines and to gather data. They are financing their own staff to conduct this research which in general is not proprietary research. It is research that is published in the open literature and work that we would do if the industry were not funding it.

Next slide, please (slide 6).

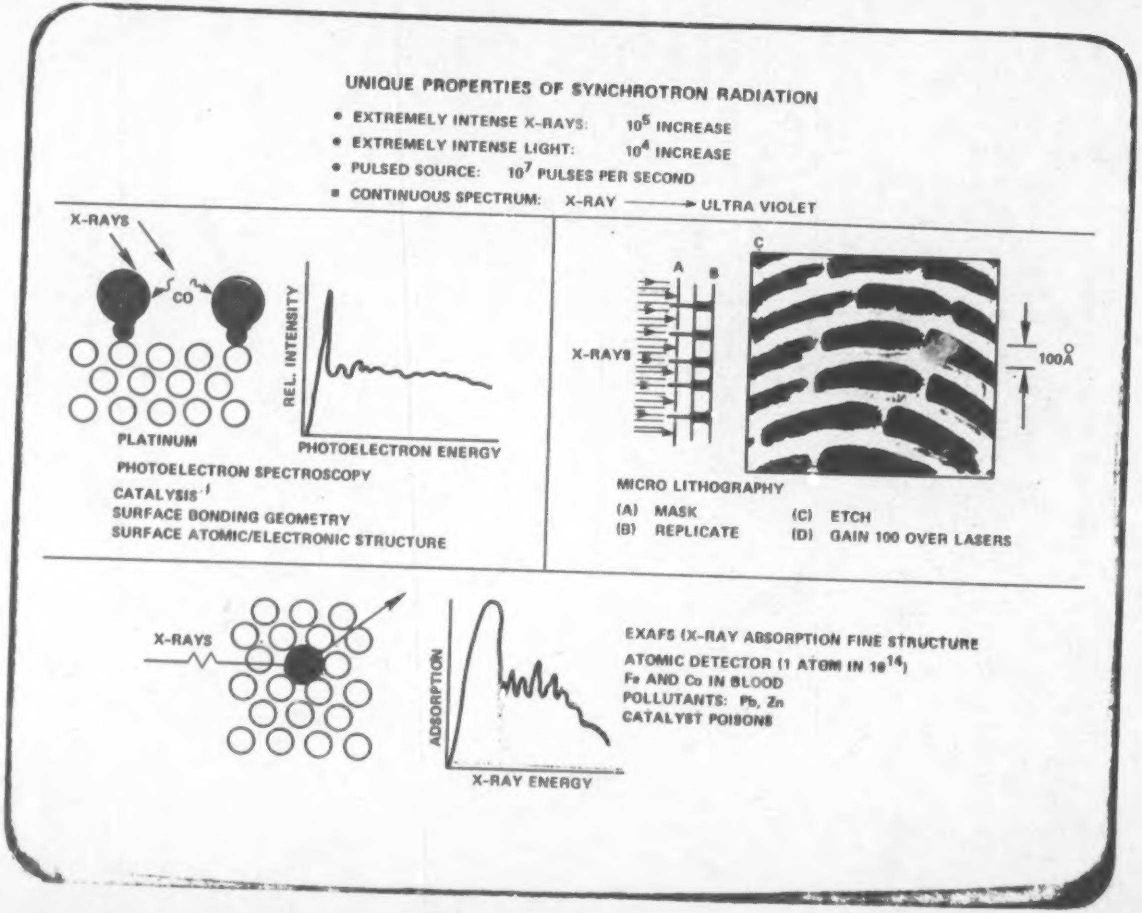

NATIONAL SYNCHROTRON LIGHT SOURCE
BROOKHAVEN NATIONAL LABORATORY
X-RAY STORAGE RING
EXPERIMENTAL BEAM LINES (28)
LABORATORY
VUV STORAGE RING
EXPERIMENTAL BEAM LINES (16)
ENTRY
LINAC
CONTROL AREA
RAMP
BOOSTER
MECHANICAL EQUIPMENT
COURTYARD
RAMP
RF EQUIPMENT
LABORATORIES
X-RAY SHIELD TUNNEL
BROOKHAVEN AVENUE
TECHNOLOGY STREET

SLIDE 6

Dr. **Kropschot.** The next slide shows a picture of one part inside that building. Within the next few weeks, we expect that active research will be ongoing using this ring to look at the nature of atoms and molecules.

Synchrotron radiation is rapidly becoming a valuable tool for a wide range of research all the way from the fundamentals of combustion and catalysis, to the study of blood, to some practical applications in electronics.

Next slide, please (slide 7).



UNIQUE PROPERTIES OF SYNCHROTRON RADIATION

- EXTREMELY INTENSE X-RAYS: 10^5 INCREASE
- EXTREMELY INTENSE LIGHT: 10^4 INCREASE
- PULSED SOURCE: 10^7 PULSES PER SECOND
- CONTINUOUS SPECTRUM: X-RAY ——→ ULTRA VIOLET

X-RAYS
CO
PLATINUM
REL. INTENSITY
PHOTOELECTRON ENERGY

PHOTOELECTRON SPECTROSCOPY

CATALYSIS
SURFACE BONDING GEOMETRY
SURFACE ATOMIC/ELECTRONIC STRUCTURE

X-RAYS
A
B
C
100Å

MICRO LITHOGRAPHY

(A) MASK
(B) REPLICATE
(C) ETCH
(D) GAIN 100 OVER LASERS

X-RAYS
ABSORPTION
X-RAY ENERGY

EXAFS (X-RAY ABSORPTION FINE STRUCTURE
ATOMIC DETECTOR (1 ATOM IN 10^{14})
Fe AND Co IN BLOOD
POLLUTANTS: Pb, Zn
CATALYST POISONS

SLIDE 7

Dr. KROPSCHOT. This slide shows several applications; however, focus on the upper right-hand corner which is labeled microlithography. The idea is to use these very short-wave length X-rays to etch out a pattern for very large-scale integrated circuits. Its impact on electronics is to permit components to be placed very close together so that thousands of elements of an electronic circuit can fit on the head of a pin. This is a way of manufacturing the electronic circuit, and the slide shows an early and fairly successful attempt at developing this kind of electronic circuitry.

The synchrotron radiation research is conducted at several locations, but it is primarily located at the synchrotron light source at Brookhaven and at the Stanford Synchrotron Radiation Laboratory at Stanford University. For the first time, in fiscal 1983, The Stanford program has been transferred to basic energy sciences. It is ongoing, and was previously funded by the National Science Foundation. I point this out to call your attention to the budget request for fiscal 1983. The basic energy sciences budget of $283 million is a 16-percent increase over fiscal 1982. That increase includes the Stanford Synchrotron Radiation Facility which is not in our fiscal 1982 budget. It is, therefore, a new responsibility that we are undertaking, and that causes part of the step increase.

Another part of the budget increase is for the implementation of the research program with the Brookhaven synchrotron light source. It has previously been a construction project. We are going to turn it on and start to use it. The operating funds to just get the light out—to just get the X-rays out of the ring—make a substantial increase in our budget. We also are facing the problem of funding new research that can be done with the synchrotron light source; and implementing that research program is a big part of the remainder of the 16-percent increase.

Overall, the budget is almost a level-of-effort for basic energy sciences when you back out those added costs. We will start some new programs by completing old ones.

As I did last year, I compiled a list of approximately 30 accomplishments, and I have provided you the titles of those accomplishments in my handout. I will also provide a list of those titles for the record. In addition to that, we have a paragraph written up that describes each of those accomplishments if you would like.

[The information mentioned above follows:]

BASIC ENERGY SCIENCES
RECENT PROGRAM ACCOMPLISHMENTS

BES

- NEW MEASUREMENT OF CALIFORNIUM-252 NEUTRON YIELD STANDARD
- THEORETICAL PREDICTION OF SOLID STATE PHYSICAL PROPERTIES
- NEW NON-DESTRUCTIVE DOWNHOLE SEISMIC SOURCE
- BASIC SCIENCE APPLIED TO FABRICATION OF CORROSION RESISTANT INTERMETALLIC MATERIALS
- LASER BASED TECHNIQUE PERMITS STUDY OF CRITICAL TRACE SPECIES IN FLAMES
- PREDICTIVE MODEL FOR THE DUCTILE-BRITTLE-TRANSITION-TEMPERATURE FOR FERRITIC STEELS
- NEW PROCESS TO RECOVER URANIUM FROM LEACH LIQUORS
- HIGHLY SPECIFIC EXTRACTANTS SYNTHESIZED FOR RECOVERY AND PURIFICATION OF VALUABLE METALS
- NEW PHOTOCHEMICAL REACTION FOR PRODUCING HYDROGEN
- INTERNAL FRICTION IN CAR TIRES — MODEL FOR ENERGY LOSS CHARACTERISTICS

BASIC ENERGY SCIENCES
RECENT PROGRAM ACCOMPLISHMENTS

BES

- ELECTROMAGNETIC MEASUREMENTS TRACK IN SITU OIL SHALE RETORTING
- VIBRATIONAL CONTROL FOR INCREASING PRODUCTIVITY OF CHEMICAL REACTORS
- PRODUCTION OF OXYGEN ENRICHED AIR USING LIQUID MEMBRANES
- USE OF MICROORGANISMS TO REMOVE SULFUR AND NITROGEN FROM FOSSIL FUELS
- HYDROGEN FLUORIDE USED TO CONVERT CELLULOSE TO SUGAR
- KEY DEVELOPMENTS IN PLANT MOLECULAR BIOLOGY
- COATED SEMICONDUCTOR ELECTRODE IMPROVES POSSIBILITY OF ELECTROCHEMICAL SOLAR CELL
- BREAKTHROUGH EXPERIMENT MEASURES ELECTRON-ELECTRON INTERACTIONS
- TWO DISCOVERIES STRENGTHEN SCIENTIFIC BASIS FOR CATALYST DESIGN
- MATHEMATICAL MODELS OF TURBULENCE

11/81

BASIC ENERGY SCIENCES
RECENT PROGRAM ACCOMPLISHMENTS

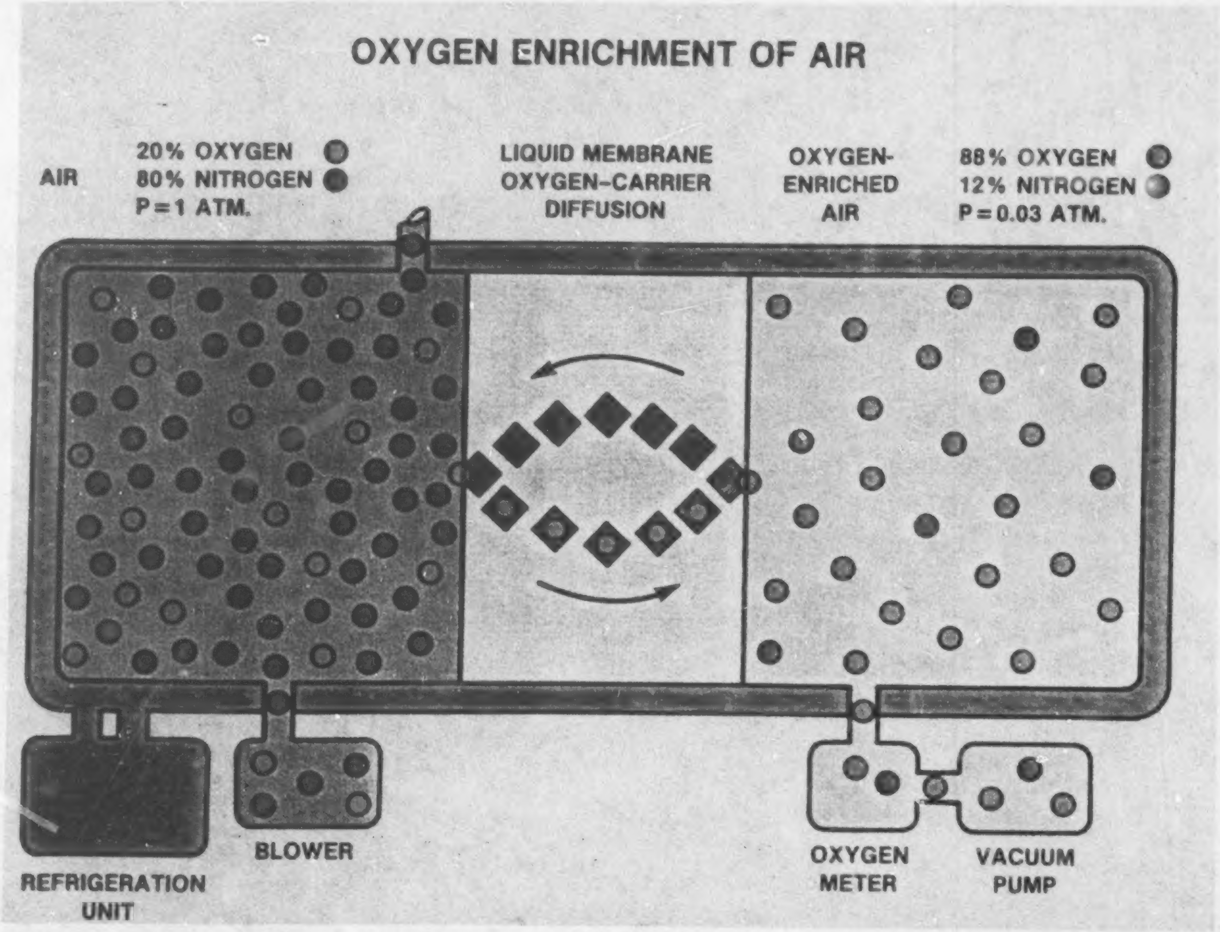
BES

- MICROBIAL SYNTHESIS OF LARGER MOLECULES FROM CARBON MONOXIDE
- MODEL TO PREDICT THE EFFECT OF RESIDUAL STRESS ON SUPER-CONDUCTING MATERIALS
- IRRADIATION STABILITY OF CERAMICS FOR RADIOACTIVE WASTE DISPOSAL
- FRACTURE IN DUCTILE METAL OBSERVED BY MICROSCOPY
- DYNAMICS OF MOLECULAR IONS NOW ACCESSIBLE THROUGH CONTROLLED COULOMBIC FRAGMENTATION
- FLUORESCENCE OF ACTINIDES: HIGHLY SENSITIVE MONITORING TECHNIQUE
- CHANGE IN CATALYST SUBSTRATE DRAMATICALLY CHANGES FUEL PRODUCT
- COAL AND OIL SHALE NON-DESTRUCTIVELY ANALYZED FOR ORGANIC MATTER
- LONG TERM STABILITY OF NUCLEAR FUELS
- STUDIES OF SULFATE POLLUTANT RELATED TO ACID RAIN SOURCES

10/81

In closing, I would like to pick up on one of those accomplishments and describe it.

Next slide, please (slide 8).



OXYGEN ENRICHMENT OF AIR

AIR

20% OXYGEN ●
80% NITROGEN ●
P = 1 ATM.

LIQUID MEMBRANE
OXYGEN-CARRIER
DIFFUSION

OXYGEN-
ENRICHED
AIR

88% OXYGEN ●
12% NITROGEN ●
P = 0.03 ATM.

REFRIGERATION
UNIT

BLOWER

OXYGEN
METER

VACUUM
PUMP

SLIDE 8

Dr. KROPSCHOT. Let me conclude by highlighting the novel process of separating oxygen out of air. In the pink area in the center I illustrate a membrane that separates oxygen from air. The membrane selectively picks up the oxygen on the left side and carries it through to the right, leaving the nitrogen behind. We can accumulate oxygen-enriched air having nearly 90 percent oxygen. This room-temperature process replaces the cryogenic techniques which take air to very low temperatures, and passes it through a distillation column, removing the oxygen and dumping nitrogen-enriched air back into the atmosphere.

The process has been displayed recently at the Energy Technology Conference here in Washington. It had fairly large interest by industrial organizations. We anticipate that now that we have completed the program, it will be picked up and carried forward by other people.

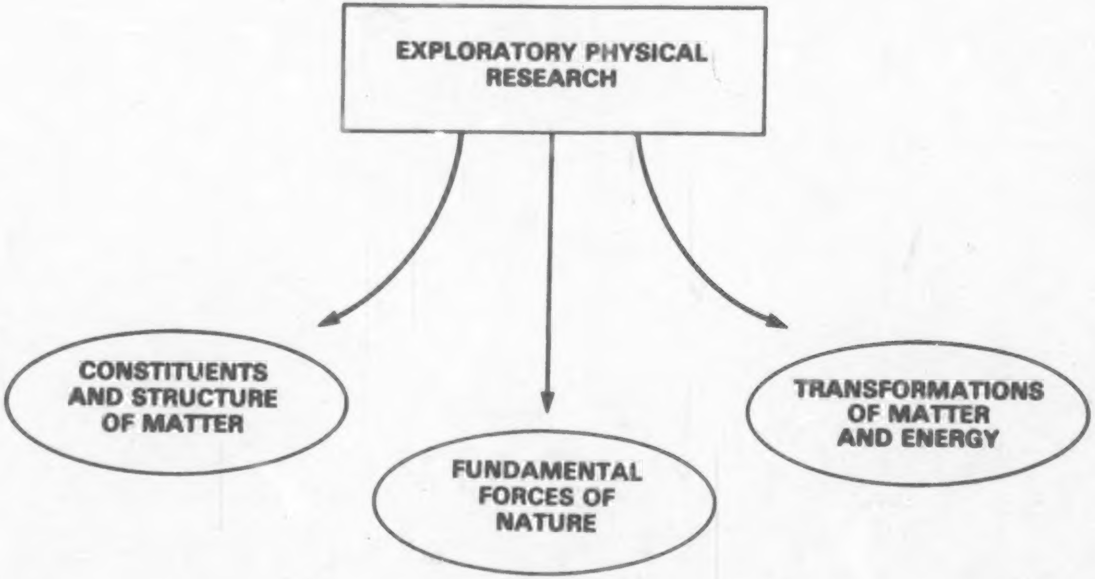
Mr. Chairman, that completes my testimony. I would like to turn the program over to Dr. Leiss.

Mr. VOLKMER. Thank you very much, Dr. Kropschot. Now, we will hear from Dr. Leiss.

Dr. LEISS. Thank you very much, Mr. Chairman. In a few minutes I would like to give you an idea of the overall high energy and nuclear physics programs of the department. Through a discussion of a number of pictures, I would like to show you some of the recent progress that has been made in this program.

Next slide, please (slide 9).

HIGH ENERGY AND NUCLEAR PHYSICS



EXPLORATORY PHYSICAL RESEARCH

CONSTITUENTS AND STRUCTURE OF MATTER

FUNDAMENTAL FORCES OF NATURE

TRANSFORMATIONS OF MATTER AND ENERGY

SLIDE 9

Dr. **Leiss.** The first slide illustrates the fact that the high energy and nuclear physics programs are an area of exploratory physical research. This research is primarily concerned with trying to understand what are the constituents of matter, how the transformations occur between matter and energy, and what are the fundamental forces of nature.

This is a study that has gone on for many, many years. Discoveries such as the electromagnetic theory of matter serve as the fundamental foundation of much of our technological society.

Next Vu-graph, please (slide 10).

FOCUS

NUCLEAR PHYSICS

- NUCLEI:

  STRUCTURE AND INTERACTIONS
- NEW FORCES IN NUCLEI
- EXOTIC FORMS OF NUCLEAR MATTER

HIGH ENERGY PHYSICS

- BASIC CONSTITUENTS:

  STRUCTURE AND INTERACTIONS
- FUNDAMENTAL FORCES OF NATURE
- UNIFICATION OF FORCES

SLIDE 10

Dr. Leiss. The focus of these programs in high energy and nuclear physics relates to what are the basic constituents of matter, their structure, their interactions, the fundamental forces of nature, and how these forces can all be described in one unified theory. This concept of a unified theory to explain all of the forces was a dream of Einstein's which he was not able to accomplish.

Recently, we have made great progress in a theory that has met all tests up to the present time. Unification of the electromagnetic and the weak nuclear force has been developed. Great progress in developing the theory to combine the strong nuclear force into this theory appears to have been made, and experiments to try and test this theory are now being made.

In nuclear physics, the questions and the focus of the program is very similar to that in high energy physics except that we are trying to understand how these forces influence the structure and the interactions of matter. What are the forces? Is the nuclear force inside the nucleus the same as it is between two nucleons outside the nucleus?

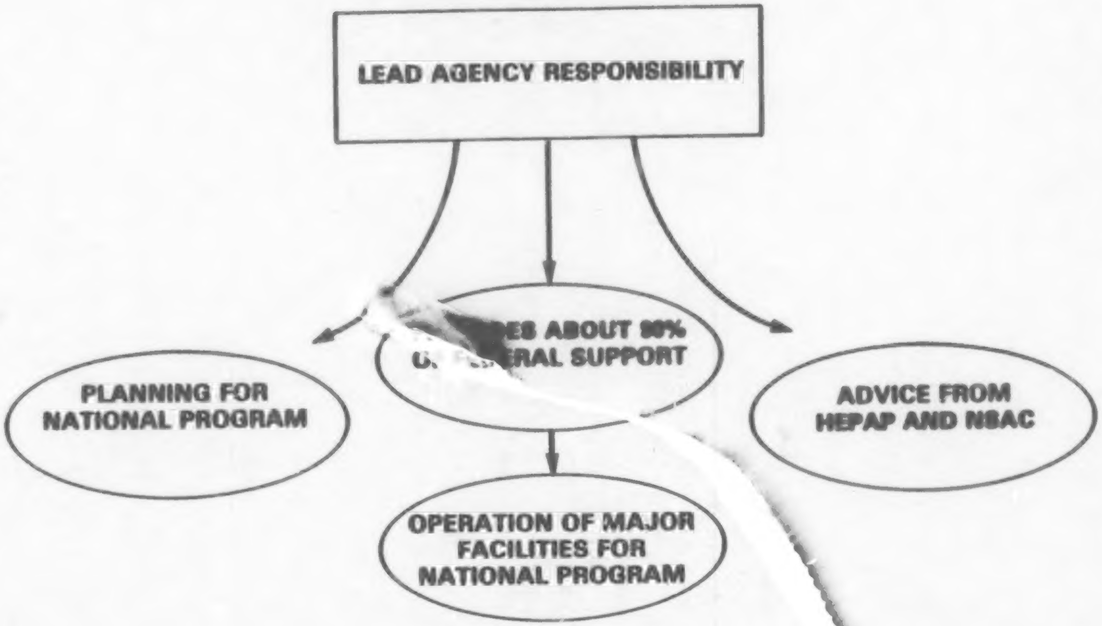
We also look in heavy ion physics experiments for exotic forms of nuclear matter. There have been predictions that if matter is compressed strongly enough in collisions, you will phase transitions will result into foreign types of matter that have never been seen by man.

These are the areas of focus of the program.

Next Vu-graph, please (slide 11).

HIGH ENERGY AND NUCLEAR PHYSICS

DOE ROLE



LEAD AGENCY RESPONSIBILITY

PLANNING FOR NATIONAL PROGRAM

[illegible]ES ABOUT 90% U.[illegible] FE[illegible]RAL SUPPORT

ADVICE FROM HEPAP AND NSAC

OPERATION OF MAJOR FACILITIES FOR NATIONAL PROGRAM

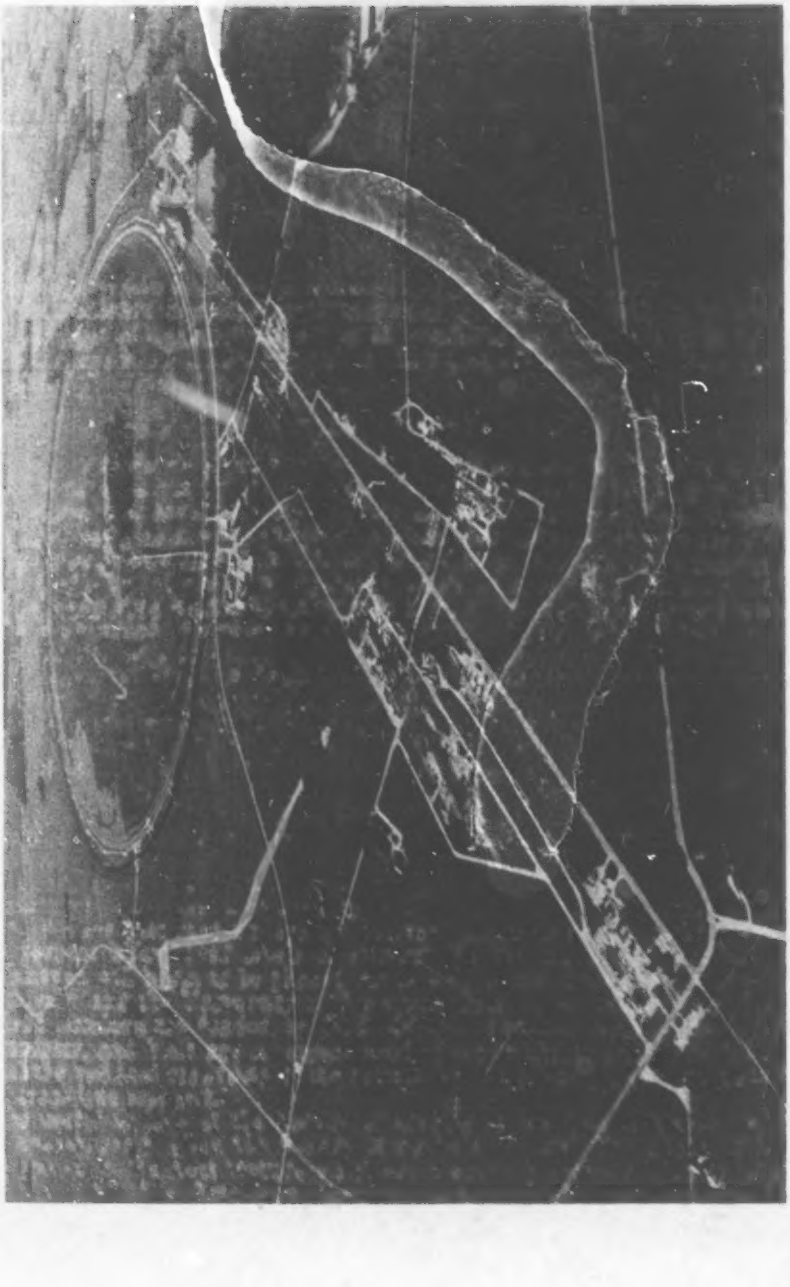
SLIDE 11

Dr. **Leiss**. The DOE provides about 90 percent of the Federal support for research in high energy and nuclear physics. Therefore, the Department serves essentially as a lead agency for the program. The remainder of the funding is provided by the National Science Foundation.

We have responsibility for planning and coordinating the national program. In doing this, we get substantial advice from advisory committees such as the High Energy Physics Advisory Panel and the Nuclear Science Advisory Panel.

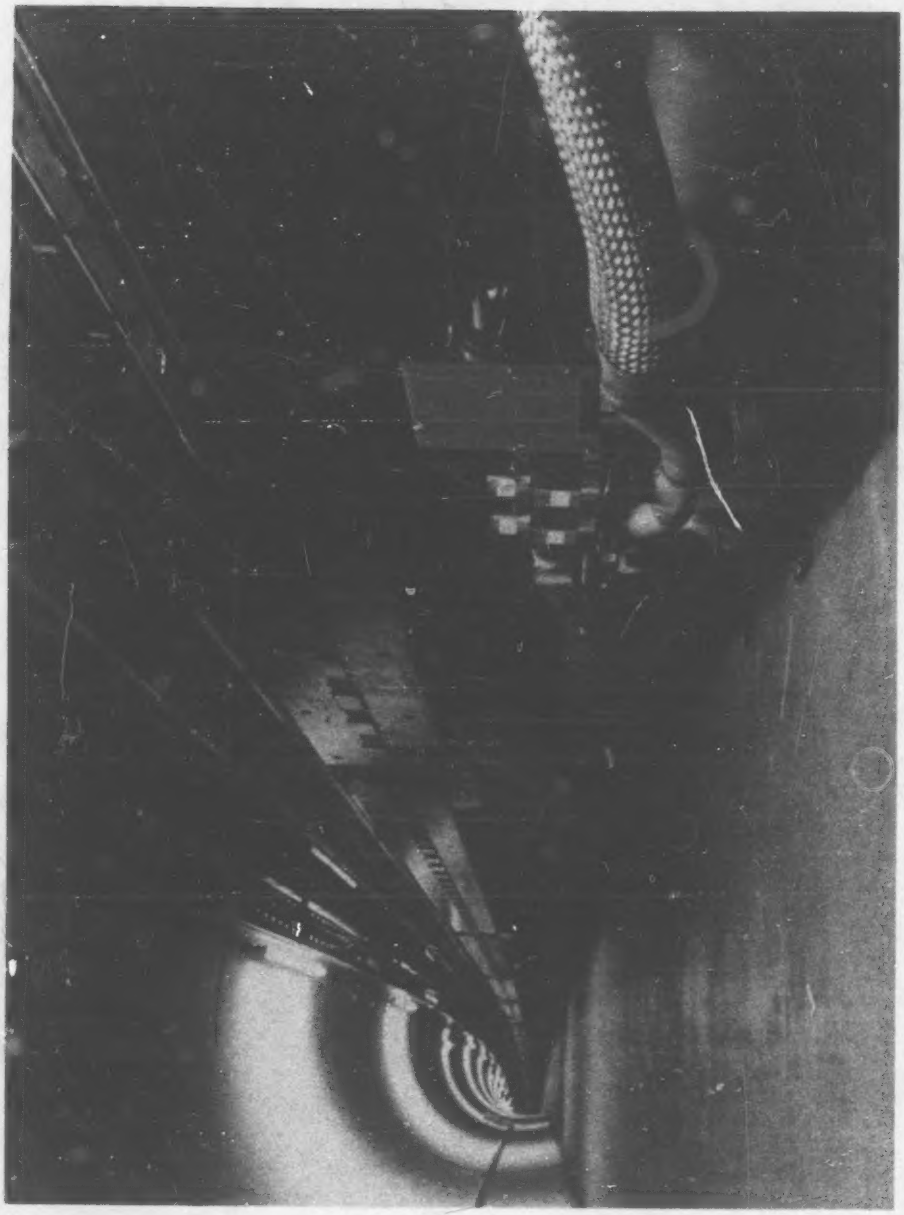
We also have the major responsibility for operation of the large facilities required for these programs. This is not exclusively true, but the major facilities are operated by the Department and made available to scientists on the basis of scientific merit of their proposals independent of where they are supported.

To illustrate some of the facilities and the progress made in the program, I would like now to show a number of pictures of facilities.



Slide 12

Dr. LEISS. The first of these is an overall view of Fermilab (slide 12) the Fermi National Accelerator Laboratory in Illinois. It is a very large facility. The ring is about 2 miles in diameter here. The next photograph is inside that ring and shows a view of some of the magnets in the tunnel of the accelerator.



SLIDE 13

Dr. **Leiss.** We have underway an upgrading program of the Fermilab (slide 13). The magnets on top are for the 400 GeV proton synchrotron that has been installed at Fermilab for a number of years. The magnets in the bottom are superconducting magnets that have been developed and are being installed as part of the energy-saver project.

I am happy to tell you that we now have some half a mile of these magnets installed and they are undergoing cryogenic tests, that is, they are being ramped in their field just as if it was an accelerator, held at a flat top and then brought down as if particles were being injected and accelerated.

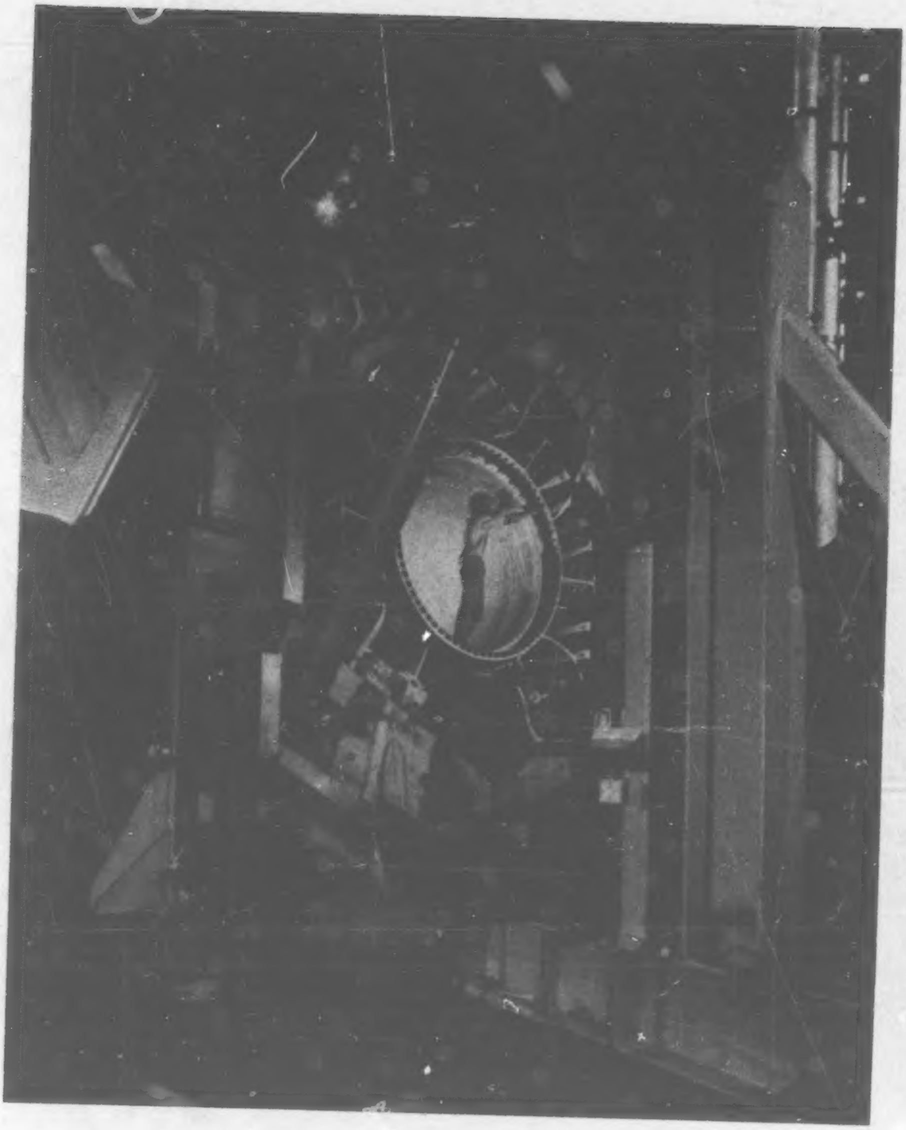
This is far and away the largest test of a superconducting system that has ever been made in the world. We are extemely pleased at the progress on this. We expect that project to be completed approximately at the end of this year with attempts next spring to start up the energy saver as an accelerator.

Dr. **Leiss.** There are two other projects based on the energy saver: Tevatron I and the Tevatron II. The first of these, the Tevatron I, allows us to bring the energy saver up to an energy of approximately 1,000 GeV and to also store both protons and antiprotons in the ring, which will allow colliding beam capability at 2,000 GeV. The luminosity, that is, the rate at which interactions occur, will be relatively low, but on the other hand, it will allow a glimpse into an unknown territory where no other accelerator can reach at this time.

The second project, Tevatron II, is also under construction. It just started. This project will bring the 1,000-GeV beams out to the various experimental areas for fixed-target experiments.

SLIDE 14

Dr. Leiss. The next vu-graph is a picture of the Stanford Linear Accelerator Center (slide 14). Inside of one of the facilities—the positron-electron colliding beam facility—PEP—is a picture—next Vu-graph, please.



SLIDE 15

Dr. **Leiss** [continuing]. Of a very large detector—a so-called time projection chamber (slide 15). I show this for two reasons. One reason is so you can see the size of the large detectors that are needed in these programs. Another reason is that this detector represents a major achievement in that we have essentially developed here the equivalent of a three-dimensional cloud chamber where all the signals are collected electronically so that one can get the three-dimensional reconstruction of many tracks from particle collisions electronically; and one can, therefore, sort through many, many events to find the rare processes of interest.

Next Vu-graph, please (slide 16).

SLIDE 16

Dr. Leiss. The next picture shows a view of the alternating gradient synchrotron at Brookhaven National Laboratory. This facility has been in operation for a number of years. There have been a number of major experimental achievements on this facility. I show this to point out that in fiscal year 1983 we expect to start using this facility with polarized protons.

We had a facility—the zero gradient synchrotron (ZGS) at Argonne—which before its operation was terminated, had polarized protons, and using the ZGS, scientists discovered a number of extremely interesting effects when one does polarized beam experiments. We now expect to have this capability at the AGS to carry on this exciting research activity.

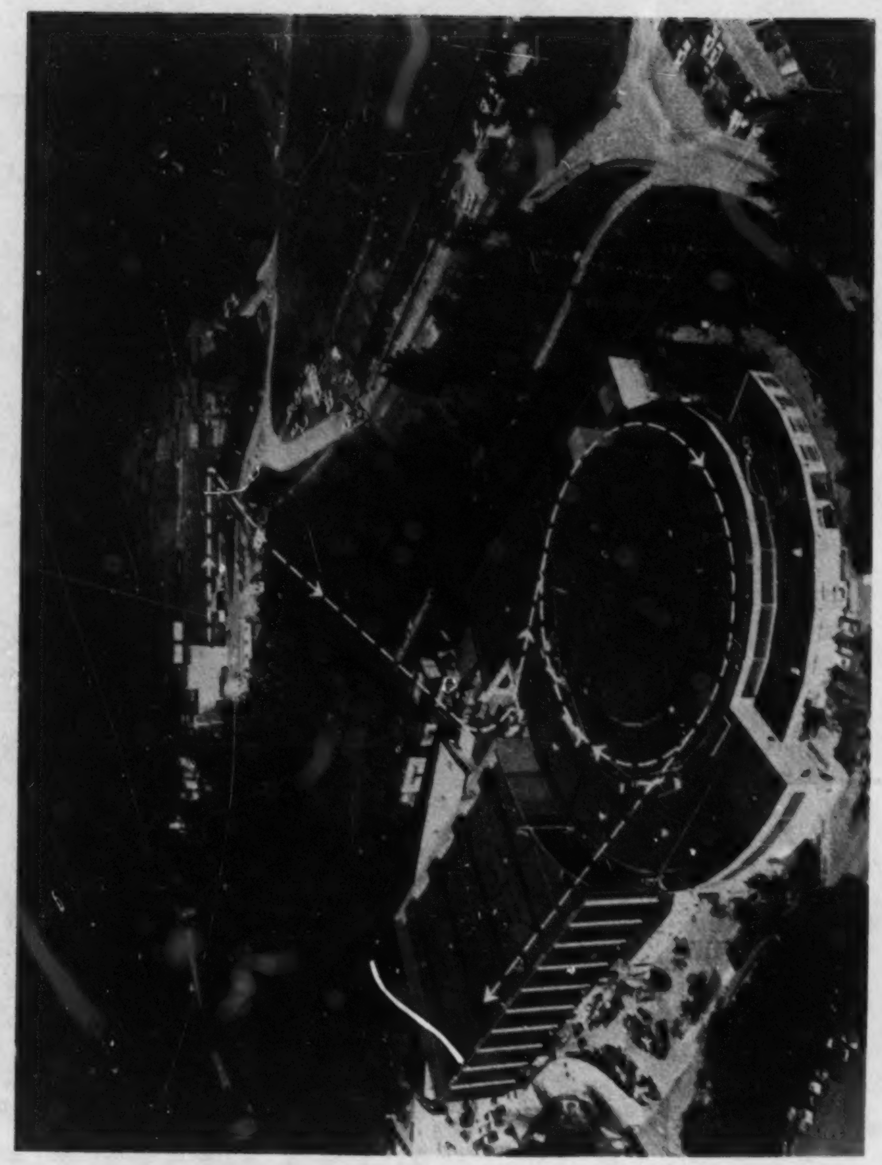
SLIDE 17

Dr. **Leiss**. I turn now to the nuclear physics program facilities (slide 17). I show first a picture of the Los Alamos Meson Physics Facility. This machine has been in operation for a number of years. Down in the left-hand corner of that picture you will see a white mound. That is part of a neutron target area which is being expanded, and a proton storage ring. It is an addition to that facility funded primarily by Defense programs and basic energy sciences. This will be another one of the applications outside of nuclear physics made of this facility, as a major source of neutrons for neutron solid state interaction studies. I think it will be the most powerful such source in existence for a number of years.

Slide 18

Dr. LEISS. The next Vu-graph is a picture of the Bates linear accelerator—an electron linear accelerator—at MIT (slide 18). We have had a construction project to take the electron beam from the end of that machine, bend it back to the front end, and reinsert it into the front end of the machine, thereby getting twice the original electron energy for a relatively small price.

I am very happy to say that over this last weekend, they have for the first time succeeded in getting that beam through the second time. It seems to be working much as expected, although there will be several months of tuneup to make it into a useful thing. They hope to start experiments toward the end of the summer using that facility.



SLIDE 19

Dr. LEISS. The next is a picture of the Bevalac at the Lawrence Berkeley Laboratory (slide 19). Another project which we have had underway for a number of years is to develop the capabilities of this facility to accelerate heavy ions of very large mass to quite high energies.

This project was completed in December of last year using very innovative techniques to achieve the ultra-high vacuum required so that the heavy ions do not get lost during the acceleration process. They are now starting experiments using krypton beams which is the heaviest mass high energy particle every accelerated. By this summer, they expect to be accelerating gold ions, so this has been an extremely successful project.

HIGH ENERGY PHYSICS

LONG RANGE PLANNING PRINCIPLES

(HEPAP SUBPANEL ON LONG RANGE PLANNING)

- IMPROVE UTILIZATION LEVELS OF PRESENT FACILITIES
- EXPEDITIOUS COMPLETION OF ON-GOING ENERGY SAVER AND TEVATRON CONSTRUCTION
- ESSENTIAL NEED FOR A NEW FACILITY WITH MAJOR RESEARCH CAPABILITY TO COME INTO OPERATION BY 1990
- CRITICAL NEED FOR ADVANCED TECHNOLOGY R&D FOR LONG RANGE PROGRAM FUTURE

SLIDE 20

Mr. LEISS. Let me skip the next two if I might and go on to my last two Vu-graphs which are related to long-range planning (slide 20).

In these types of programs you require very large facilities and large pieces of equipment, as I think the various slides that I have shown to you illustrate. That and the good scientists in the program are our major resources. The resources are expensive. They take a long time to develop. Therefore, we pay a great deal of attention to the question of long-range planning, trying to understand what are the budgetary and programmatic implications to the activities that we undertake.

We recently have had a HEPAP subpanel on long-range planning, the so-called Trilling Committee, that has just completed their report, and this report has been transmitted to us by the High Energy Physics Advisory Panel.

They concluded that the major planning principles upon which the program needs to operate over the next several years are, first, to improve the utilization of our present facilities which has been allowed to fall to a rather dangerously low level. Second is the expeditious completion of the ongoing construction of the energy saver and Tevatron facilities. Third is the essential need which the high energy physics program has for a new facility with major research capability to come into operation in approximately 1990. Fourth is the critical need for advanced accelerator technology R. & D. so that when we think about the machine that we want to come into operation in the year 2000 we will have some idea of how to build it, what kind of a facility it ought to be, and how to get the cost down to something that the Nation can afford. These are the major principles that have to guide our high energy physics programs.

NUCLEAR PHYSICS

LONG RANGE PLANNING PRINCIPLES

- IMPROVE UTILIZATION LEVELS OF PRESENT FACILITIES TO MEET NEEDS OF RESEARCH PROGRAM
- CONSTRUCT HIGH DUTY FACTOR, HIGH ENERGY ELECTRON ACCELERATOR FACILITY FOR HIGH PRIORITY RESEARCH IN THE 1990's
- UPGRADE EXISTING FACILITIES AND INSTRUMENTATION AT RATE MATCHED TO ADVANCES IN THE SCIENTIFIC PROGRAM
- PROVIDE SUFFICIENT LEVELS OF SCIENTIFIC MANPOWER TO CONDUCT RESEARCH REQUIRED BY THE FIELD

SLIDE 21

Dr. **Leiss**. Similarly, in nuclear physics we operate with long-range planning principles (slide 21). Again, there is the need to improve utilization and therefore the cost effectiveness of our investments in our present-day facilities which is seen as a major need.

We see a strong need for the construction of a high-duty factor electron accelerator that would come into operation toward the end of this decade. We have been funding now for several years R. & D. related to how to build that particular machine.

We need to upgrade our facilities and instrumentation in all of our laboratories at a rate commensurate with the needs of the scientific program, and we need to pay continuous attention to the scientific manpower needs of this field, just as in all other fields of science.

HIGH ENERGY AND NUCLEAR PHYSICS

CONTRIBUTIONS OF TECHNOLOGY

- SUPERCONDUCTING MATERIALS AND DEVICES
- RF POWER SOURCES
- SYNCHROTRON LIGHT SOURCES
- MEDICAL APPLICATIONS
- UNIVERSAL INSTRUMENTATION STANDARDS
- RADIATION PROCESSING
- NATIONAL DEFENSE

SLIDE 22

Dr. LEISS: I would like to close with a slide speaking to the contributions of our programs to technology (slide 22). The fundamental justification of the high energy and nuclear physics programs is the basic science that is done in these programs in the quest by mankind to understand nature. However, in the process of people trying very hard to learn how to do these things that we do not have the tools to do and that very smart people are trying to do, we end up developing a lot of technology that finds applications in other areas.

I think we can, in fact, take credit for major developments in superconducting materials and devices, microwave power sources, the synchrotron light source that Dr. Kropschot mentioned, as an outgrowth of the high energy and nuclear physics programs, and the many medical applications, and so forth.

With that, I close and turn the program back to Dr. Trivelpiece.

Dr. TRIVELPIECE. Mr. Chairman, I would like to close with some comments. The world in which we live is becoming progressively more technologically oriented. Our ability to compete as a Nation in the international marketplace, I believe, is going to depend in part on how well we do in various complex high-technology arenas.

I cannot conceive of a long-range strategy for a vigorous U.S. economy that does not include a strong component of Government support for various areas of basic long-range research and development at our universities, national laboratories, and in industry.

I believe that the programs that we have discussed here today are elements of this strategy. They have been singled out for strong support by the administration even in these times of budgetary stringencies. I certainly hope that they also have your strong support.

Thank you, Mr. Chairman. We will be happy to answer any questions.

[The prepared statement of Dr. Trivelpiece follows:]

Statement of Alvin W. Trivelpiece
Director
Office of Energy Research
Department of Energy
Subcommittee on Energy Development and Applications
of the
House Science and Technology Committee
March 30, 1982

Mr. Chairman and Members of the Subcommittee:

I am pleased to appear before you today to discuss the FY 1983 budget request for several of the research programs supported by the Office of Energy Research. The programs that I will discuss today include High Energy Physics and Nuclear Physics under the General Science and Research appropriation, and Basic Energy Sciences, Energy Research Analyses, University Research Support and Multiprogram General Purpose Facilities under the Energy Supply R&D appropriation.

The research programs supported by the Office of Energy Research are important investments in our Nation's future. I believe that a vigorous program of scientific research contributes to national security, economic growth and, in no small sense, to human culture and understanding. Our ability to solve our economic and technological problems in this decade and beyond will depend on the scope and depth of our scientific knowledge and on the progress that results from continued support for basic research programs.

Let me turn now to the budget request for our programs. Table 1 summarizes the Energy Research request for the programs that I will discuss today. Under General Science and Research, the FY 1983 budget request for High Energy Physics is $429.0 million. The request for Nuclear Physics is $131.0 million; and we are requesting $2.0 million for General Sciences Program Direction. Under Energy Supply R&D, our FY 1983 budget request includes $282.9 million for Basic Energy Sciences, $2.6 million for Energy Research Analyses, $5.1 million for University Research Support and $34.0 million for Multiprogram General Purpose Facilities. Our request also inlcudes $3.2 million for Advisory and Oversight Program Direction and $0.4 million for Policy and Management.

I would like to discuss each of these programs briefly, beginning with those under the General Science and Research appropriation, and then I would be pleased to respond to questions.

Table I

Office of Energy Research
Summary by Program
($ in millions)

	FY 1982 Estimate	FY 1983 Estimate
General Sciences		
High Energy Physics	$364.5	$429.0
Nuclear Physics	133.9	131.0[a]
General Sciences Program Direction	1.9	2.0
Energy Supply - R&D		
Basic Energy Sciences	$243.3	$282.9[b]
Energy Research Analyses	3.0	2.6
University Research Support	10.6	5.1
Multiprogram General Purpose Facilities	32.6	34.0
Advisory and Oversight Program Direction	3.3	3.2
Policy and Management	1.0	0.4

a/ Reflects transfer of National Superconducting Cyclotron project to National Science Foundation (NSF); prior to transfer, $6.8 million was planned for the project in FY 1983.

b/ Reflects transfer from NSF of Stanford Synchrotron Radiation Laboratory, resulting in increase of $6.8 million in FY 1983 request.

HIGH ENERGY AND NUCLEAR PHYSICS

The High Energy and Nuclear Physics (HENP) programs are following lines of inquiry that have occupied man's mind throughout history. As a result of continuing research in high energy and nuclear physics, we now know more than ever before about the universe in which we live, the fundamental constituents of matter and the laws of nature that underlie all physical processes. Energy Research programs fund approximately 90% of the nation's research in high energy and nuclear physics, with the remainder supported by the National Science Foundation. These programs have been extremely successful over the last 2-3 decades. HENP-supported researchers have won five Nobel prizes in the last seven years and fifteen in the last 25 years.

During budget hearings every year, there is considerable discussion about the large laboratory and university facilities--accelerators, colliding beam facilities, detectors--that are required to carry out research in high energy and nuclear physics. Today, I would like to focus for a moment on another key term in the High Energy and Nuclear Physics dictionary. That term is "user group." Generally the experimental research in High Energy and Nuclear Physics is done by university-based scientists who frequently collaborate, or form user groups, with scientists from other universities or DOE's national laboratories.

The term user group, while descriptively accurate, does not adequately convey the importance of the concept either in terms of research or the training of tomorrow's scientists. Let me take a moment to elaborate on that concept. The size and complexity of a typical High Energy Physics research experiment makes it especially appropriate for users from a number of institutions to collaborate. These research teams design an experiment at their home institutions, then submit a proposal to a scientific program advisory committee made up of representatives from the labs and university community who evaluate the proposal and assess its relative priority for scarce accelerator time. If approved by the committee and the management of the appropriate lab, the research team fabricates, installs and tests the apparatus at the lab. Laboratory staff, in addition to constructing, operating and carrying out research on the accelerators, also provide significant assistance to the user group at this stage of the process. Experiments are carried out with laboratory assistance, and the recorded data is taken back to the home institutions for analysis and publication. The entire process from conception to publication generally takes from three to five years. Some experiments are also conducted by accelerator laboratory scientists who must compete for accelerator time in the same way as the user groups do.

The High Energy Physics program, focused at three major accelerator laboratories, supports over 100 user groups. The Nuclear Physics program, carried out at eight national facilities, accommodates approximately 70 user groups of university researchers. Through research and planning activities participants from a total of about 100 universities all across the country contribute to the success of the HENP programs. These user groups and the staffs of the laboratories represent the all important human side of these basic research activities that rely on the most expensive high technology facilities.

HIGH ENERGY PHYSICS

Let me turn now to the FY 1983 budget request for the High Energy Physics Program. The FY 1983 budget request for High Energy Physics is $429 million in budget authority. Of that amount, $331 million is for operating expenses; $47.5 million is for capital equipment and $50.5 million is for construction (Table 2). These funds will enable us to continue to seek a better understanding of the basic constituents of matter and the fundamental forces of nature that comprise and control the world around us.

In this century, scientific investigation has focused on ever more fundamental constituents of matter, and on more elemental concepts. The research frontier first moved from molecules to atoms to nuclei to the constituents of nuclei (protons and neutrons). Recently it has moved to even more elemental subnuclear constituents, such as "quarks", "leptons" and "bosons".

To probe into the depths of matter, into the domain of quarks, scientists use intense beams of very energetic particles which can be produced only by high energy particle accelerators. Progress in High Energy Physics is dependent on the availability of these forefront accelerator and colliding

Table 2

High Energy Physics
Budget Authority
($ in Millions)

	FY 1982 Estimate	FY 1983 Estimate
Operating Expenses	$ 269.2	$ 331.0
Capital Equipment	40.7	47.5
Construction	54.6	50.5
Total High Energy Physics	$ 364.5	$ 429.0

beam facilities and complex detectors that map the effects of particle collisions. The smaller the particle to be studied and the higher the energies holding its component parts together, the larger and more powerful must be the accelerators, detectors and other equipment required to do the research.

This technology, however, is only one part of the program. Progress in High Energy Physics research is based on a continuing interplay of advancements in experiment, theory and technology. High Energy Physics experiments are done at the forefront of theoretical and technical knowledge; and the range of experiments possible is determined by the available technology. Physicists frequently have to invent unique equipment to carry out the experiments of greatest importance. It is in this way that High Energy Physics contributes directly and immediately to the technological base of the country.

This technological advancement, while not the goal of the research, is a virtually guaranteed bonus which cannot be predicted in advance. There have been many spin-offs from High Energy Physics research, including the technology of superconducting materials and devices, radio frequency power generation, materials analysis and medical applications, including new methods of treating cancer. More recently, the use of synchrotron light from particle accelerators opened up totally new research capabilities in chemistry, solid state physics, biology and industrial applications. Free-electron lasers, driven by particle accelerators, promise new capabilities for enhanced laser power and control. Radiation processing, which was born from this science, has many exciting new applications ranging from the manufacture of heat-shrinkable electrical insulation to the sterilization of wastes and the preservation of food.

Because of the length of time required to obtain research results and to assess the true significance of these results vis a vis other research results, the specific end product of the research--the new knowledge--cannot readily be planned for or predicted in advance. Despite this, the history of progress and achievement in this field enables us to say confidently that we are moving rather rapidly toward our goal of better understanding the physical world around us.

For example, we have an increasingly better understanding of the so-called basic forces in nature. It appears now that the previously assumed four forces of nature--electromagnetic, weak nuclear and strong nuclear forces, and gravity--may indeed be different forms of a single unified force. Research results have indicated that the weak nuclear and electromagnetic forces are actually a single force--the electroweak force. The 1979 Nobel Prize went to two U. S. physicists for work toward this unification. The 1980 Nobel Prize in Physics also was given to two U. S. physicists for work that, ultimately, may unify the strong nuclear and electroweak forces into a "grand unified force," as it is referred to. This would reduce the number of forces to only two. Current thinking is that the gravitational force will be incorporated into a unification scheme, thus bringing all of the basic forces into a single unified framework, a life-long goal of Albert Einstein. We cannot predict what will follow from research aimed at verifying this unification theory. However, we can anticipate profound, world-changing effects analogous to those that followed the unification of electricity and magnetism by Maxwell in the late 19th century and the unification of matter and energy by Einstein early in this century.

The FY 1983 budget request of $429 million for High Energy Physics compares to $364.5 million in FY 1982. This increase of $64.5 million would permit us to pursue these important lines of investigation with a significantly higher level of utilization of existing facilities. It also would provide for continued progress on the Fermilab development program as well as long-range accelerator R&D. More specifically, it would allow us to increase facility utilization at Fermilab, the Stanford Linear Accelerator Center (SLAC) and at the Brookhaven National Lab (BNL). It would also allow us to provide additional support for university and laboratory groups at levels matched to the higher utilization level of facilities and the greater complexity of experiments.

The high energy particle accelerator and colliding beam facilities that would be operated under the requested FY 1983 budget include the 30 billion electron volt (GeV) proton Alternating Gradient Synchrotron at Brookhaven, the 500-1000 GeV proton synchrotron at Fermilab and the 32 GeV linear electron accelerator at SLAC. SLAC also has two electron-positron colliding beam facilities for which the linear accelerator acts as the injector. These facilities are called SPEAR and the Positron-Electron Project (PEP). Each of the three national accelerator centers provides a unique set of capabilities which complements the others. Together, they represent the facilities essential for a comprehensive national research program in High Energy Physics.

The FY 1983 budget request for operating expenses of $331 million is necessary to increase the low facility utilization levels experienced in recent years as a result of fiscal restraints, rapidly increasing power costs and the construction of new or upgraded accelerators. The FY 1983 request would allow us to increase utilization levels that are currently below 50% to the 60-70% range. Another major feature of the operating program in FY 1983 is the shakedown, initial operation and achievement of the first circulating beam in the Energy Saver modification at Fermilab. Energy-Saver is the first step of an ongoing development program aimed at increasing the power of the proton synchrotron at Fermilab while, at the same time, reducing the cost of electricity required to run accelerators. The Energy-Saver, which consists of a ring of superconducting magnets installed inside the main ring tunnel, will extend operation of the accelerator to 500 GeV or higher for fixed target experiments. It is these superconducting magnets that operate at temperatures of nearly absolute zero that allow major savings in electric power costs.

The operating expenses request also will give priority to operations at PEP, the electron-positron colliding beam facility at SLAC. PEP began operation in late 1980. In FY 1983, the full initial complement of detectors, including the High Resolution Spectrometer and the Time Projection Chamber, will be in operation.

The construction request of $50.5 million includes $36 million for continuation of the Fermilab development program, $18 million each for Tevatron I and II. Tevatron I, will permit acceleration up to 1000 GeV and provide a facility for proton-antiproton collisions at 1000 GeV. Tevatron II will further expand the accelerator's capabilities by providing upgrading of the experimental areas and beam lines tangential to the main ring for fixed target experiments. The remainder of the construction request is for Accelerator Improvement Projects essential to maintain the reliability, efficiency and scientific effectiveness of the existing accelerator facilities and for General Plant Projects essential to the efficient and reliable operation of the laboratory sites.

Budgets for High Energy Physics have fallen short of long-term planning levels in recent years and especially in FY 1982. These shortfalls have been absorbed primarily by substantial reductions in the level of facility operations and by cutbacks in the rate of construction of facilities, especially on the ISABELLE project. No construction funding for the ISABELLE project is requested in FY 1983. Difficulties have been encountered in the development of suitable superconducting magnets for this project. While considerable and encouraging progress has been made during the last year, substantial effort is still needed to develop a prototype capable of being produced at acceptable costs in the number required. The R&D efforts supported at Brookhaven allow for development of suitable magnets and for the analysis of alternate accelerator options which, while retaining unique and exciting physics research potential, can be constructed within available budget resources.

Let me turn now to the Nuclear Physics program.

NUCLEAR PHYSICS

The FY 1983 budget request for Nuclear Physics is $131 million, with $113.4 million of that for operating expenses, $9.9 million for capital equipment and $7.7 million for construction (Table 3). Through the Nuclear Physics program we continue to seek a better understanding of atomic nuclei and nuclear matter at the most fundamental level possible. By using nuclei as a "proving ground" we also hope to learn more about the fundamental forces of nature--a goal Nuclear Physics holds in common with High Energy Physics.

Table 3

Nuclear Physics
Budget Authority
($ in Millions)

	FY 1982 Estimate	FY 1983 Estimate
Operating Expenses	$ 112.4	$ 113.4
Capital Equipment	9.5	9.9
Construction	12.0	7.7
Total Nuclear Physics	$ 133.9	$ 131.0

If we were to achieve these goals completely, we would be able to quantitatively describe all of the diverse structures, properties and interactions of nuclei in terms of the fundamental forces and constituents of our universe. Although we may never completely realize these goals, their pursuit does lead us to a better understanding of nuclei and their role in the physical universe. In doing so, it gives us a national base of knowledge, techniques, instrumentation and skilled manpower in critical technologies.

The FY 1983 budget request would provide for a continuing program of closely related experimental and theoretical investigations of atomic nuclei. Basic nuclear physics research has a growing interaction with high energy particle physics and astrophysics. In FY 1983, these growing interface areas of nuclear physics will be emphasized. As our understanding of quarks grows it is becoming clear that quarks and their interactions must play an important role in the structure, internal dynamics and interactions of nuclei. However, the nature of this role is an open scientific issue. This issue would be explored through theoretical and experimental investigations in FY 1983, investigations that will include a look at the possibility of new and very different forms of nuclear matter.

Nuclear physics, high energy particle physics and astrophysics also share an interest in the weak interaction. One of the fundamental forces of nature, the weak interaction governs the beta decay of nuclei. In FY 1983, research at the Los Alamos Clinton P. Anderson Meson Physics Facility

(LAMPF) and at MIT's William H. Bates Electron Linear Accelerator would focus on experiments which are intended to provide understanding aimed at the unification of the strong, weak and electromagnetic forces of nature. Work is also being done with neutrinos at Brookhaven. Because neutrinos are the only form of radiation that can escape the interior of the sun without many changes, they are our only direct source of information on the physical processes of the sun's interior. In addition to improving our understanding of the unified theories of the fundamental forces, increased knowledge about neutrinos could lead to a better understanding of the origins of our universe.

Also in FY 1983, the high quality beams and detector systems now available at the Holifield Heavy Ion Research Facility at the Oak Ridge National Laboratory will be used to study the mechanisms by which colliding nuclei exchange mass, energy and spin. Completed last year, the Holifield facility will conduct its first full year of research in FY 1982.

The FY 1983 budget request of $131.0 million for Nuclear Physics is a decrease of $2.9 million from the FY 1982 appropriation. That decrease results in part from the transfer of the National Superconducting Cyclotron Laboratory construction project to the National Science Foundation. It was funded at $4.5 million for FY 1982.

As is the case in High Energy Physics, the capabilities of accelerator facilities determine which experiments can be conducted and which cannot. The LAMPF and Bates facilities and the Bevalac at the Lawrence Berkeley Laboratory are unique in the world and serve well their respective areas of basic nuclear research. Efficient operation of these facilities will be given high priority in FY 1983. Because of budgetary constraints, the number and mix of low energy and heavy ion facilities is undergoing reevaluation so that the new and unique capabilities of the Holifield Heavy Ion Research Facility and the Argonne Tandem/Linac Accelerator System (ATLAS), now under construction, can be fully utilized.

The FY 1983 construction request of $7.7 million for Nuclear Physics includes $3.7 million to complete construction of ATLAS. This new facility will provide heavy ion beams of exceptional quality in a previously unexplored energy range. To accommodate this evolution and assure a balanced and productive program within the FY 1983 budget, research of lesser priority at several other facilities will be reduced or possibly phased out.

Mr. Chairman, let me briefly comment on General Sciences Program Direction.

GENERAL SCIENCES PROGRAM DIRECTION

The FY 1983 budget request for General Sciences Program Direction is $2.0 million. These funds are requested to provide for the salaries, benefits, travel and other expenses associated with 36 full-time equivalents in the Office of the Associate Director for High Energy and Nuclear Physics, the Division of High Energy Physics, the Division of Nuclear Physics, Life Sciences Research and Nuclear Medicine Applications program, and associated support staff. The staffing requirements remain constant relative to FY 1982. The increased funding is to provide for normal increased personnel costs such as within grade and merit increases, and benefits.

I would now like to discuss the programs under the Energy Supply R&D appropriation, beginning with Basic Energy Sciences.

BASIC ENERGY SCIENCES

The Basic Energy Sciences program conducts long-term research to provide a fundamental base of scientific and engineering knowledge for use by government and the private sector to develop new concepts, materials and processes important for energy production and use. Support of this basic research is an investment in our nation's future economic well-being, both in terms of the knowledge produced and the training of university students who will be tomorrow's energy experts.

To say that the BES program sponsors research in the physical and biological sciences, engineering and mathematics does not convey the magnitude of a program that supports approximately 1100 research projects a year and provides advanced research facilities that are used by university and industrial researchers as well as national laboratory scientists. Nor does it convey the impact of a research program whose benefits cannot be predicted or scheduled. While we cannot predict the specific long-term impact of most basic research activities, experience during the past few decades makes us confident that the cumulative effect of research underway will be a wealth of new information that will lead to new and improved energy technologies and will enable us to address problems with present energy systems.

The many accomplishments flowing from research carried out by this program during the last year include:

o Development of a new method for fabricating a class of less brittle materials called intermetallic compounds. These compounds have excellent corrosion resistance which is important for energy applications including automotive and industrial uses. In the past, they have seen limited use because of their brittleness.

o Discovery of a new class of chemical compounds that can be used to recover and purify selected valuable elements, such as palladium from nuclear waste solutions.

o Discovery of a liquid membrane to enrich the oxygen content in air. Oxygen is important for efficient combustion, and crucial for high Btu coal gasification. The new liquid membrane is an improvement over state-of-the-art solid membranes; with it, oxygen-enriched air could be produced at great savings in energy and cost compared to present day technology.

- o Discovery of a micro-organism that is capable of converting the waste gas, carbon monoxide, into commercially-valuable, larger molecular weight chemicals such as acetic acid. This conversion, which can be accomplished without energy input from other sources, could provide a more economical process for the chemicals and plastics industries among others.

- o Development of a combustion process diagnostic technique that, by improving our understanding of flame constituents and characteristics, will help build our ability to tailor fossil fuels to meet operating requirements of combustion systems and reduce pollutant emissions.

This latter example, the initiation of research on combustion phenomena, is an excellent example of the dynamic nature of the Basic Energy Sciences program. The BES program does not have a static technological end point toward which it strives. Rather, the program is responsive to new research opportunities and needs that flow both from ongoing research and from work in new areas. The need for combustion research was first identified several years ago when DOE was established. Working with members of the research community and the Department's technology programs, the general needs for combustion research were identified.

It was determined that the longer range technologies would benefit from an improved understanding of combustion processes, including identification of the species present in flames and their concentrations, chemical interactions at the atomic and molecular levels, and a better understanding of fluid dynamics and turbulence in flames and combustion systems. With the needs and opportunities for research on the fundamentals of combustion identified, we undertook design and construction of a unique facility at our Sandia-Livermore Laboratory. This facility was designed around the special experience in laser diagnostics that had been developed at the Laboratory to serve nuclear weapons needs. The Combustion Research Facility (CRF) was dedicated during 1981 and has been designated as a user facility where DOE-supported scientists from various locations, university researchers and privately-supported industrial researchers alike can conduct research. Work at the CRF has implications not only for improved fuels, but for higher efficiency engines, cleaner and more efficient coal combustion, and knowledge on the extent and future control of pollutants.

Other promising areas of research supported by BES have wide-ranging applications. Energy storage needs associated with solar energy, electric vehicles, and central station power plants are being investigated by BES-sponsored research in a number of areas, including chemical reaction kinetics, solid electrolytes, high temperature thermodynamics, electrodes and electrochemistry, high temperature corrosion, and diffusion and gas transport. While these areas of fundamental research are, no doubt, foreign to the average person, I can assure you that investigations into their domain are essential if we are to make significant improvements in our current technologies and develop new ones. Similarly, in response to the need for long-term safe isolation of nuclear wastes, BES is supporting research in radiation effects on materials, thermodynamics of ceramics and brines, fluid flow in porous media, canister materials, actinide chemistry, mechanical and transport properties in geologic systems and other areas. This research provides important additional information to help us resolve issues related to nuclear waste management such as permanent disposal, interim storage, and separation of nuclear waste components.

In order to maintain the kind of high quality research I have just described, we must continue the strong core program that now exists. We must also be able to expand research in emerging priority areas not yet included in the core program as well as maintain, operate and update the major facilities required to carry out the research.

The FY 1983 budget request for Basic Energy Sciences is $282.9 million (Table 4). Of that amount, $261.3 million is for operating expenses; $18.2 million is for capital equipment; and $3.4 million is for construction. This is an increase of $39.6 million over the FY 1982 appropriation for this program. However, 17% of this increase is for the Stanford Synchrotron Radiation Laboratory (SSRL), currently supported by the National Science Foundation. The budget increase reflects, in part, a $6.8 million interagency transfer for SSRL. At the requested funding level for FY 1983, most of the ongoing core programs would be supported although some reductions would still be required in the larger subprograms so that we can support increases for major facility operation and research. Although some program expansion

Table 4

Basic Energy Sciences
Budget Authority
($ in millions)

	FY 1982 Estimate	FY 1983 Estimate
Operating Expenses	$226 3	$261.3
Capital Equipment	16.4	18.2
Construction	0.6	3.4
Total	$243.3	$282.9

would be possible in the newer subprograms of Biological Energy Research, Advanced Energy Projects and Engineering, Mathematics and Geosciences, most of the increase would support the operation of BES's major research facilities, including the SSRL.

Adequate support of the BES scientific facilities is vital to the productivity of these programs. These facilities include not only the Combustion Research Facility, but synchrotron light sources, research reactors, the Intense Pulsed Neutron Source and large electron microscope facilities at the DOE laboratories. These research facilities provide capabilities that are, in most cases, unique in the U.S. and in some cases in the world. They are available to the agency's technology programs as well as to a wide spectrum of the scientific, academic and industrial communities. The costs of maintaining, operating and updating these facilities have been steadily increasing as a result of escalating power costs, significant increases in costs for fabricating new nuclear fuel elements and costs for operating newer facilities as they are completed. Additional funds requested for FY 1983 are mainly to operate the major facilities near their FY 1982 levels, although funding for the newer National Synchrotron Light Source, the Intensed Pulsed Neutron Source and the Combustion Research Facility still would not allow operation at optimum levels.

The FY 1983 budget for BES includes a number of programmatic shifts of funding and management responsibility. As mentioned earlier, the BES program will assume full budget and management responsibility for the

Stanford Synchrotron Radiation Laboratory previously supported by the National Science Foundation. SSRL is a national user research laboratory associated with the High Energy Physics facility known as SPEAR located at the Stanford Linear Accelerator Center (SLAC). In addition to serving the High Energy Physics program, the SPEAR facility provides synchrotron radiation to SSRL for research in materials and chemical sciences, physics, engineering, biology and basic and applied subfields.

The transfer is part of the Administration's effort to improve management and planning for national user facilities by assigning to a single agency lead responsibility for a facility. The shift brings support for the two major synchrotron radiation user facilities, SSRL and the National Synchrotron Light Source at Brookhaven National Laboratory, into a single appropriation.

The second programmatic change involves the transfer of all low energy nuclear physics research from the Basic Energy Sciences program to the Nuclear Physics program. This change transfers $12.8 million in FY 1983 from BES to the Nuclear Physics program and represents a management decision to consolidate both management and budget responsibility for all basic nuclear physics research in a single program. The nuclear data and isotopes production activities remain in BES.

Finally, the budget for BES includes shifting of operational and funding responsibility for research reactors to specific single subprograms within BES. Previously, funding responsibility was shared by several other ER programs. This change streamlines responsibility and management accountability for the research reactors.

I will now describe the Energy Research Analyses, University Research Support, and Multiprogram General Purpose Facilities programs. The FY 1983 budget request for these programs is shown in Table 5 following my discussion of the programs. Before I discuss these outlay progams, however, I would like to comment on the advisory responsibilities of the Director of Energy Research.

ADVISORY AND ASSESSMENT ACTIVITIES

The Director's advisory responsibilities include serving as the principal science advisor to the Secretary on energy R&D programs, on oversight of the non-weapons multiprogram laboratories and on the education and training of scientists and engineers. The Office of Energy Research also provides staff support to the Energy Research Advisory Board (ERAB) whose members advise the Secretary on the Department's energy R&D programs. The Board's membership includes representatives from industry, universities, energy users, state and local governments and public interest groups. ERAB now reports directly to the Deputy Secretary of the Department of Energy. Staff support is provided through ER's Director of Science and Technology Affairs Staff.

Within the Office of Energy Research, the Office of Field Operations and the Office of Program Analysis (OPA) support the Director's advisory responsibilities. The Office of Field Operations Management supports the Director with regard to oversight of the laboratories and energy-related manpower development and training activities. This Office has developed several management tools used by the Department in administering its laboratories and R&D facilities across the country and continues to manage the University Research Support and the Multiprogram General Purpose Facilties outlay programs that I will discuss shortly. The Office of Program Analysis, on the other hand, provides energy research analysis support.

ENERGY RESEARCH ANALYSES

The FY 1983 budget request for Energy Research Analyses is $2.6 million (Table 5). The Office of Program Analysis (OPA) administers the Energy Research Analyses program and provides technical support to the agency through the Director of the Office of Energy Research. The functions and activities of this program include:

1. Assessment of the adequacy of the long-range (technology base) research in support of the agency's programs to develop energy options.
2. Independent analyses and technology assessments of the agency's research and development programs to guarantee their quality and relevance.
3. Coordination and liaison for resolution of major technical issues and research policy developments.

Typical of the technology base assessment function was the assemblage of a prestigious group of industrial and university scientists to assess the long-range research needs in coal gasification, coal liquefication and oil shale. These assessments have been used by the agency to develop R&D programs, by the Synthetic Fuel Corporation to assess project readiness and by industries to define their own research agendas. Similar studies addressed biomass and plant-derived hydrocarbons, salinity gradient solar ponds, materials research needs to improve performance, and durability of solar heating and cooling systems.

Independent analyses of programs such as alcohol fuels, use of methanol for automotive transportation and electrochemical storage were carried out during the past year to provide management with a realistic assessment of their technological status and potential. In addition, OPA has provided an in depth review of the quality and relevance of research programs supported by the Office of Energy Research, including Basic Energy Sciences and Health and Environmental Research.

Activities related to the interagency coordination of difficult technical issues focused on acid rain in FY 1982. Currently, OPA assists in the planning of agency-supported acid rain research, coordinates acid rain issues and activities within the agency and represents the agency on the Interagency Task Force on Acid Precipitation. Also, OPA has initiated the development of a goal-oriented comprehensive R&D plan for all Federal acid rain research.

The FY 1983 budget request would provide for: (1) approximately five ER-initiated major technology area assessments; (2) an update of portions of the agency's Technology Base Assessment; and (3) selected technology studies as requested by senior DOE management.

UNIVERSITY RESEARCH SUPPORT

The agency's major contribution to university education and training of new professional-level manpower is through the funding of the highest quality individual research proposals involving both faculty members and graduate students. We estimate that over 6,000 graduate students currently are supported by DOE, either directly on research projects or indirectly through the laboratories. The agency is also a significant user of technical manpower. Its 57 plants and laboratories alone utilize over 100,000 contractor employees. It is our specific policy to support energy research and training programs aimed at the utilization of unique agency laboratories and equipment by both university faculty and students. We also provide support for refueling specialized university nuclear research and training reactors located across the country. These activities, in addition to the direct support of university researchers provided by our own basic research programs, continue to be appropriate and effective energy research education and training programs. They are aimed at securing top quality energy researchers and at broadening the pool of experienced scientists from which Government, universities and industry draw trained scientific manpower. The FY 1983 budget request of $5.1 million, a reduction of $5.5M from FY 1982, would concentrate these efforts in two high priority areas (Table 5).

The first is the University Reactor Fuel Assistance Program, which supports university nuclear research and training reactors on university campuses. The program specifically provides support for the fabrication and related shipping costs of the nuclear fuel used in university reactors. There currently are 54 such reactors in operation in the U.S. Without our support, most of these reactors would be hard pressed to continue their dual functions of nuclear research and the training of undergraduate and graduate students in nuclear science and engineering. The FY 1983 request of $1.6 million would allow us to fuel up to five reactors plus associated spent fuel shipping and partial support for reactor operational costs when these reactors are "shared" with researchers at neighboring schools.

The other high priority URS activity is the University-Laboratory Cooperative Program. This program (along with user research facilities) is one of the most important mechanisms we have to sustain the close working relationships between our National Laboratories and the academic community. Through this program we support a range of faculty and student research participation activities at all of our major Laboratories. In many cases, these are summer research programs, while in other cases research appointments for faculty members and graduate students are made for an entire academic year. The requested budget for this program in FY 1983 of $3.5 million will support an estimated 2,000 university participants throughout the Laboratory network. Special emphasis again will be placed on providing research opportunties for engineering faculty and students.

MULTIPROGRAM GENERAL PURPOSE FACILITIES

The Multiprogram General Purpose Facilities (MGPF) program, initiated in FY 1981, provides a mechanism for rehabilitating and replacing non-defense general support facilities which are essential to the continued effective and safe operation of the multiprogram laboratories. These facilities include roads, railroads, site utilities and support buildings such as laboratories and office buildings.

The 12 multiprogram labs serviced by this program received over $3.1 billion in FY 1981 to carry out the agency's technology and basic research programs. The replacement costs of the existing facilities exceeds $8 billion. A backlog of $2 billion worth of GPF improvements has developed over the years. This program has begun to address that backlog and has taken management steps to assure that the problems responsible for the backlog will be corrected.

During stringent budget years it is often tempting to postpone necessary replacement or rehabilitation of deficient facilities in favor of maintaining the maximum amount of program effort possible. We have learned from the experience of our predecessor agencies that such expediencies lead to negative impacts on the very programs they aimed to protect and also result in an inventory of billions of dollars in corrective measures which some future generation will be expected to undertake. We plan, therefore, to continue the MGPF program, which began in FY 1981, as a necessary cost of doing business as long as the multiprogram laboratory facilities are utilized to perform research and development functions for the agency.

The FY 1983 request of $34.0 million would allow continued construction on ten projects started in prior years and this year and would fund initiation of four critical new projects in FY 1983 (Table 5). This represents a slight increase over the $32.6 million appropriated in FY 1982.

ADVISORY AND OVERSIGHT PROGRAM DIRECTION AND POLICY AND MANAGEMENT

The FY 1983 budget request for Advisory and Oversight Program Direction is $3.2 million, a net decrease of $0.1 million from the FY 1982 level. These funds are required to provide for the salaries, benefits, travel, and other expenses associated with 41 full-time equivalents funded by this program. This staffing level represents a decrease of 15 full-time equivalents from the FY 1982 level. The decreased salary requirements associated with that staffing reduction are partially offset by increased requirements such as normal salary and benefits increases for remaining staff, and severance pay.

The FY 1983 budget request for Policy and Management is $0.4 million, a decrease of $0.6 million from the FY 1982 level. These funds are required to provide for the salaries and related expenses associated with 8 full-time equivalents in the immediate offices of the Director, Office of Energy Research, and the Director, Office of Management. This staffing level represents a reduction of 14 full-time equivalents from FY 1982.

Mr. Chairman, this concludes my testimony. I would be pleased to answer any questions at this time.

Table 5

Energy Research Analyses, University Research Support,
Multiprogram General Purpose Facilities
Budget Authority
($ in millions)

	FY 1982 Estimate	FY 1983 Estimate
Energy Research Analyses	$ 3.0	$ 2.6
University Research Support	10.6	5.1
Multiprogram General Purpose Facilities	32.6	34.0

Mr. **Volkmer**. Thank you very much, Dr. Trivelpiece. There are quite a few questions that I have and I am sure other members and the staff will have some. I will start off and then let them continue. Then we can come back to me again. I hope we will not take too long.

I would first like to ask you why is it desirable to maintain all three of our high energy physics centers: the Stanford Linear Accelerator Center, the Fermilab, and Brookhaven? Why do we need to maintain all three of those?

Dr. **Trivelpiece**. I can think of many answers to the question. I am not sure what prompted it. Let me begin by saying that the need for various kinds of tools by which the substructure of nuclei can be investigated is important. You need to have the ability to have heavy particles—heavy particles such as protons on protons. You need to have the ability to have electrons impinge on electrons and other combinations thereof in order to have the tools necessary to explore that particular region of subatomic matter.

Each of these facilities performs one of those functions. That, to me, justifies their existence.

Mr. **Volkmer**. Will Brookhaven be able to continue with only their present facilities as they presently have?

Dr. **Trivelpiece**. Well, as we indicated earlier—I guess, we didn't indicate it earlier—the construction at Brookhaven for the Isabelle project is not going forward at this time. We have recommended that they be given the resources necessary to do the research and development on the superconducting magnets to determine whether or not that project can continue at some time in the future.

In the meantime they have the facilities that Dr. Leiss referred to, and they are still continuing to do beneficial research.

Mr. **Volkmer**. But for how long would you say? How long would they be able to continue? Let's say that the only facilities are the present facilities that they have.

Dr. **Trivelpiece**. I don't know. Perhaps, Dr. Leiss would like to answer that.

Dr. **Leiss**. Well, the AGS facility, as you know, has been in operation for more than 20 years. They certainly have provided surprises by making new discoveries over the course of time. However, I think it is clear that as the facilities get older and new facilities are built, the AGS will not be competitive with other facilities. As I said, it is very dangerous to predict when because you keep getting fooled.

Mr. **Volkmer**. Yes.

Dr. **Leiss**. However, I think that certainly by the end of this decade the AGS would probably have outlived its usefulness.

Mr. **Volkmer**. What is the——

Mr. **Carney**. Would the gentleman yield at that point?

Mr. **Volkmer**. Just a moment. I want to ask a question. What is approximately the use factor right now—the percentage of use right now—at the AGS lab at Brookhaven?

Dr. **Leiss**. In fiscal year 1982—well, let me get it in percentages as you asked for. We expect to operate the AGS at 45 percent of the available time. In fiscal year 1983, in line with our belief that the utilization of our facilities needs to be raised, we are projecting an operation of approximately 75 percent of the time for that facility.

Mr. VOLKMER. Is the 45 percent because of lack of funds or because of lack of demand?

Dr. LEISS. It is primarily funding. On all of our programs, the operation is funding limited.

Mr. VOLKMER. It is funding. Yes.

Dr. LEISS. I am sure that you recognize that we have had to cope not only with normal inflation in our programs, but these machines are all energy eaters and the more-rapid-than-normal-inflation escalation of power bills has been very, very difficult for us in recent years.

Mr. VOLKMER. I understand that. Can you tell me which of the three—if you can't today, could you provide this to us at a later time—high energy physics laboratories serve the greatest number of outside users?

Dr. LEISS. The Fermi National Laboratory certainly has the largest number of outside users at the present time, sir.

Mr. VOLKMER. All right.

Dr. LEISS. It also has the largest operating budget.

Mr. VOLKMER. Should that be a factor—should the number of outside users be a factor in determining continuity or continued support for the laboratory, whatever laboratory it is?

Dr. LEISS. I think that certainly is one of the factors. You know, the scientists try to go where the most exciting physics is. Therefore, where the users go is determined by two things: one, the scientific interest; and, two, the capability of a particular facility to handle users. Therefore, it is certainly one of the factors, but it cannot be used blindly.

Mr. VOLKMER. All right. Now, NSF will continue to have some high energy physics programs even with the transfers that we are talking about. Is that correct?

Dr. LEISS. That's correct.

Mr. VOLKMER. Are the NSF and DOE programs complementary? Do they complement each other in their operations or are they separate and distinct?

Dr. LEISS. In high energy physics, the DOE funds about 92 percent of the national program. The remaining 8 percent is funded by the National Science Foundation. The DOE operates the three major facilities at Fermilab, Brookhaven, and SLAC. The NSF funds the electron positron storage ring at the Cornell University, and it funds some of the users of both that facility and the facilities operated by DOE with an emphasis on university support from the NSF side.

The Department also funds university support at approximately the same level as NSF does. This is a mechanism of more than one funding source. General guidelines on what the focus of each agency's program is but not rigid rules, together with close interaction and joint use of advisory committees by the two agencies is in my opinion a very great strength of the American system of support of science. I strongly believe that it is a very valuable thing for America as long as appropriate coordination between the agencies occurs, as I believe it does in our programs.

Mr. **Volkmer**. All right. Thank you. Now, we have heard this morning that by 1990 we need a new and better facility. I am sure that thereafter there will have to be an additional facility somewhere down the line again. We all know of the budget restraints that we have, and I am sure those are going to continue for a few more years. Is anybody looking at the possibility or are there any plans that are going to be made any international cooperation in constructing these facilities?

Dr. **Leiss**. Sir, there has been an international committee looking into the general question of an international facility for a number of years now. As you know, trying to put together a large international project in science is an extremely difficult and time-consuming affair. People have been looking at it and in fact trying to understand what the problems are. We certainly have now a substantial degree of collaboration with other countries in the use of our facilities. We have a number of Europeans, Japanese, and Chinese using our facilities in America now. We in turn are major users of the two major European facilities at DESY and at CERN. I think this is a step and a very important step in developing such collaborations.

The step from there to building a truly international machine is a very big step.

Mr. **Volkmer**. Would it be safe to say that at the present time and in the near future there is no possibility of an internationally constructed facility?

Dr. **Leiss**. I believe that it will be a number of years down the road before one can talk about that. We do, however, pay considerable attention to the facilities that are being built by other countries in deciding what we should do and conversely the Europeans do so as well.

Dr. **Trivelpiece**. I would like to add to what Dr. Leiss said. I think there will occur a time at which the cost of a facility may well suggest that international cooperation is the only way to proceed to the next step. However, the conditions that would drive that are not yet here, but I am sure they will occur eventually.

Dr. **Leiss**. Yes; I agree with that.

Mr. **Volkmer**. All right. Dr. Trivelpiece, does the fiscal year 1983 DOE request for high energy physics reflect a proper balance between physical research, facility operation, facility construction, and long-range R. & D. for the future generation of facilities?

Dr. **Trivelpiece**. Well, I believe so.

Mr. **Volkmer**. You don't think that we need any additional funds replaced or changed in the structure that we presently have within the budget?

Dr. **Trivelpiece**. Not at the moment, sir.

Mr. **Volkmer**. All right. At this time, I will recognize the gentleman from New York.

Mr. **Carney**. Thank you, Mr. Chairman. Dr. Trivelpiece, we looked at the utilization figures for the AGS at the Brookhaven National Laboratory, and you indicate that it was used 45 percent in fiscal year 1982. You indicated that it will be used 75 percent in fiscal year 1983. I think that a significant part of that upgrading is the effort, certainly, by DOE, but I do not think that anyone can overlook the fact that New York State made an enormous move to help preserve that laboratory by providing additional electricity at a much reduced rate. They are wheeling power from the Fitzpatrick Nuclear Power Plant in upstate New York down to Long Island through a consortium of various power grids to provide that lab with electricity. I think it was an enormous gesture on the part of the New York State government to keep the AGS going and to enhance the probability of having the ISABELLE project completed, knowing the fiscal constraints that the Federal Government has. I think it is necessary to put that on the record at this time.

I would like to go back to one of the slides. "The Long-Range Planning Principles" was the name of the slide. The third part of that was an essential need for a new facility with major research capability to come into line by 1990. Keeping that in mind, is it possible that an international agreement, an international consortium as such, could provide that type of facility by 1990?

Dr. **Leiss**. Not in my opinion, sir.

Dr. **Trivelpiece**. I would agree with that. I think that the length of time required to plan such a facility would mitigate against anything being brought into existence by 1990.

Mr. **Carney**. What is the Department of Energy looking for, then, as far as a new facility by 1990?

Dr. **Leiss**. As you know, sir, there has been a recent study, the so-called Trilling panel, which has looked into the long-range planning. They recognized that within the budget guidance in which we are planning the present program, it was going to be very difficult to contain the Isabelle project within that budget without substantial damage to the overall national program. The panel made very clear that they felt that the ISABELLE project was the project of choice for that.

Now, what we have asked Brookhaven to do, in addition to completing the R. & D. to demonstrate that they can in fact reproducibly and at reasonable cost, mass-produce these magnets, is to take a very hard look at possible ways to reduce the cost: examine various options of the facility to go at Brookhaven. We also feel that other laboratories should take a look, too.

I think it would be very surprising if that facility did not end up utilizing the substantial investment that has been made at Brookhaven. However, the panel felt at that time that until they see what comes up from these R. & D. activities and the proposals, it would not be proper to make a judgment at this time.

Mr. **Carney**. Is it safe to say, then, that what was intended to be the project in 1990 was ISABELLE?

Dr. **Leiss**. Yes, sir.

Mr. **Carney**. I read the Trilling report. Indeed, I am not a high energy physicist. However, I did not get the impression that they were not convinced that the technical problems were not solved. I am somewhat confused.

Dr. **Leiss**. Perhaps I can explain that, sir. The previous magnet using superconducting braid that Brookhaven was trying to develop ran into many serious problems. There were some nine problems altogether.

When they developed the new magnet, the so-called Palmer magnet, using a superconductor cable, and they changed the method by which the magnet is clamped, essentially seven of those problems went away.

The remaining problems still have to be demonstrated. They have not achieved; for example, the field quality required in the individual magnets, although we and the panel had substantial belief that these would be resolved in the near future.

However, after you have that done, as we have learned very painfully, there is the problem of learning how to mass-produce these magnets so that the field and the field quality is exactly the same to very high tolerances over 1,000 magnets.

I believe that this is a substantial effort, and that part remains to be done in their program.

Mr. CARNEY. The two things, then, are the field of quality and the fact that they could mass-produce the magnets that would have that field of quality?

Dr. LEISS. I believe those are the principal things. They would also have to do that at a reasonable cost. I believe those are the remaining things to be done which have proven very difficult.

Mr. CARNEY. Do we not have a catch-22 here, though, by discontinuing construction money and not giving them an ability to prove their capabilities? We have a facility that, one, if we are to put it in mothballs, we have to have some construction to do that. Two, in order for them to demonstrate properly that they can meet those last two parameters that the Trilling committee was so concerned with, and indeed should have been, we are not giving them that opportunity.

If there is the demonstration of that, there would be a break in procurement abilities for at least a year and maybe more from the standpoint of not having on hand the materials necessary to go into a production phase.

We would also lose onhand—and I am certainly not trying to protect an entire construction team or a team that is involved in the completion of ISABELLE—most of that team.

It is my strong belief that for a small commitment of $10 million we can go on with certain aspects of the construction, protect an already $130 million investment, keep that team together, give them the ample opportunity to demonstrate that they indeed can fulfill the requirements of the nine parameters, and demonstrate their ability to produce the magnets in a mass-production stage at a fair cost to the Government for its investment.

We would then have accomplished something: One, protect our original investment; and, two, what I think is most significant, we would have fulfilled the third part of the long-range planning principles, and that is to provide the need for a new facility, or to answer the need for a new facility, I should say, with major new capabilities to be online by 1990.

I think that with $429 million budget we could find $10 million for construction to give the facility that chance.

Dr. LEISS. I believe, sir, regarding the one point of being able to prove their capability to develop these magnets, the funding that is being provided out of the operating budget, which is $23 million, is adequate funding to solve and demonstrate these magnet performance requirements.

Mr. **Carney**. It will not give them an opportunity to put a line on that can amply demonstrate that. It will not give them an opportunity to have facilities to put a sextant on line—I guess that the "sextant" is a word that no one likes to hear when you are talking about ISABELLE.

I think if we are to demonstrate it, we should be able to go to that extent. For an additional $10 million, I think that we are looking at this in a manner that may be pennywise and pound foolish in not giving them that opportunity.

If I could, I would also like to ask some questions pertaining not only to ISABELLE but to some other facilities as well.

Primarily, it is my understanding that you are going to discontinue funds on the tandem Van de Graaff at Brookhaven. Is that correct?

Dr. **Leiss**. Sir, in the nuclear physics program—the heavy ion physics program—we have two new facilities, the Holifield heavy ion facility at Oak Ridge National Laboratory and the Atlas facility at Argonne Laboratory. In Holifield's case, it is just completing construction. In Atlas' case, it will be completed in the near future.

We see that these facilities will take over the lead as providing user capabilities for forefront research in this area.

The double MP tandem at Brookhaven is an outstanding facility, but we do see that unless it is upgraded at some point in the future it will stop being competitive. We are trying very hard to avoid terminating operation of that facility before it is appropriate to do so.

In fiscal year 1983, it is necessary to reduce the support of that facility to stay within budget; however, that will still allow that facility to operate approximately 4,500 hours in fiscal year 1983, which in my opinion is not bad.

Mr. **Carney**. DOE has in its hand a proposal, I am led to believe, to upgrade the facility. Is that true?

Dr. **Leiss**. Yes, sir. It has several proposals that are being considered for new facilities in nuclear physics.

Mr. **Carney**. Is there any indication that you can give to this panel of what the status of that proposal is and how soon you would be in a position to make a policy commitment one way or the other?

Dr. **Leiss**. The Nuclear Science Advisory Committee met within the last week or so on these facility proposals. We do not have their formal report to us yet. Therefore, I am not prepared at this time to speak to their recommendations.

Mr. **Carney**. I can fully understand that. Are you prepared to give us a target date as to when you might be able to speak to the recommendation?

Dr. **Leiss**. I think that we will have their recommendations probably within the month.

Mr. **Carney**. Perhaps this would be a premature question to ask, but if the recommendations were such and you did upgrade the facility, wouldn't that enhance certain applications to Isabelle?

Dr. **Leiss**. That was essentially a bonus proposed by the Brookhaven group as part of that proposal. I am not sure that NSAC felt that was the primary thing to consider in reviewing that proposal.

Mr. **Carney**. I understand that, but we could get an additional bonus out of it.

Dr. **Leiss**. That is noise on the system, sir.

Mr. **Carney**. Well, let me ask you this: We are told by the users at the Brookhaven double MP tandem facility that it is in such great demand that a waiting list now exists for potential new users. Is that correct?

Dr. **Leiss**. Yes, sir.

Mr. **Carney**. What would the present users and those awaiting access to the facility do in the case that the facility was terminated?

Dr. **Leiss**. We would try our very best, as would the other facilities, to make the other facilities available to them. As I said, with the Holifield facility coming into operation and the Atlas facility coming into operation, this will provide new updated capabilities for the users. We would expect them to be attracted to these facilities.

Mr. **Carney**. I was wondering, Dr. Leiss, going back to the question that my colleage from Missouri had asked, if you could provide for the record a list of outside users of all three major labs so we as a committee could have a better understanding of that?

Dr. **Leiss**. Yes, sir.

[The information follows:]

HIGH ENERGY PHYSICS

CURRENT DOE OUTSIDE-USER PARTICIPATION

IN EXPERIMENTS AT DOE ACCELERATOR LABORATORIES

Institution	Fermilab	SLAC	BNL
1 ANL	x	x	x
2. Arizona	x	x	x
3. Brandeis			x
4. Brown	x	x	x
5. Caltech	x	x	
6. UC/Davis	x	x	
7. UC/Intercampus		x	
8. UC/Irvine			x
9. UCLA		x	
10. UC/Riverside		x	
11. UC/Santa Barbara	x	x	
12. Carnegie Mellon	x		x
13. Colorado	x	x	
14. Duke	x	x	x
15. Florida State	x	x	x
16. Harvard	x	x	
17. Hawaii	x	x	
18. N. Illinois	x		
19. Illinois	x	x	x
20. Indiana	x	x	
21. Iowa State	x	x	
22. LBL	x	x	
23. Lehigh	x		
24. Maryland	x		
25. Massachusetts			x
26. MIT	x	x	x
27. Michigan	x	x	x
28. Minnesota	x		x
29. Mt. Holyoke			x
30. Northwestern	x	x	
31. Oak Ridge	x	x	
32. Ohio State	x		
33. Oklahoma	x		
34. Pennsylvania	x		x
35. Princeton	x		x
36. Purdue	x	x	x
37. Rice	x		x
38. Rochester	x		
39. Rockefeller	x		
40. Rutgers			x
41. Stanford		x	
42. Tennessee	x	x	
43. Tufts	x	x	x
44. Virginia Tech		x	
45. Washington	x		
46. Wisconsin	x	x	
47. Yale	x		x
TOTAL	37	28	21

Mr. CARNEY. I have one last question, if I may, Mr. Chairman. Getting back to the synchrotron light source at Brookhaven National Laboratory, how much of that program will be utilized by outside people?

Dr. KROPSCHOT. We expect heavy involvement from the university and industrial community. About 20 percent of the activities will be conducted by the Brookhaven staff, and the rest will be by outside users.

Mr. CARNEY. You mentioned industrial users and university users. Would both those types of people be using that on a cost-sharing basis or would there be any revenues generated by the utilization of that equipment in those two areas?

Dr. KROPSCHOT. At this point, we have two opportunities to use it. One is cost sharing and one is not. We do not have a request to use it on a cost-sharing basis. The program committee for the laboratory reviews the request for research. It prioritizes the research in terms of opportunities and needs, and it assigns beam time and port area on the basis of the research.

The agreement is that the research will be published, and it will be available to all. Therefore, it will be research that we would like to do had we funds to do it, and it does in that sense give us collaboration and cooperation.

We have developed, along with our patent people, the opportunity for an industry to come in and pay full cost recovery if they want to do proprietary research. At this point, to the best of my knowledge, we have not had a request for that type of function of the facility.

Mr. CARNEY. I appreciate your answer to that question. I keep bouncing from one facility to the other, and I do not mean to do that, but I would like to go back again to the tandem Van de Graaff or the double MP tandem facility.

I do have a concern about those people who are using it. In particular, I know that MIT has expressed a great deal of concern that they do not see any relief if that facility was to be closed down. I say that not in the form of a question, but in the form of a statement. I would hope that if that is the intention that you would look not only at MIT but at other users of the facility to insure them that we can find facilities that they can use.

Mr. Chairman, I would simply like to close by thanking you for giving me the opportunity to participate in today's hearing. I was a member of this subcommittee in the 96th Congress, and I took on additional responsibilities in other committees in the 97th Congress.

I would like to simply close by saying that the people in New York have made a great commitment to Brookhaven National Laboratory, and we feel that it is very important that the Isabelle project go forward. We have mentioned the commitment which was made by New York State. We know that there is a $130 million investment already out in the ground. I affectionately refer to it as a 2.4-mile concrete donut that sits in my congressional district. I think that we have a responsibility to protect that concrete donut and the $130 million investment.

I would hope that the DOE would go back and reevaluate their position in the fiscal year 1983 budget and look more favorably upon that project because, indeed, it is the one that answers the need for the 1990. It is the project that will provide for the high energy physics community. It is a new instrument, a new device, to fill the gap that will indeed be a reality in the 1990's.

Thank you very much, Mr. Chairman.

Mr. **Volkmer**. That is quite all right. I enjoy having you here. You bring a lot of information, and your questions are very worthwhile for the subcommittee.

I would like to continue with some of the questions on Isabelle specifically that have been brought up by the gentleman from New York.

At the present time we do not know for sure whether Isabelle will be continued or not. Is that correct?

Dr. **Leiss**. Yes, sir.

Mr. **Volkmer**. All right. Now, if it cannot continue, you said that there is a need for a new major facility in 1990. We agree on that.

Dr. **Leiss**. Yes, sir.

Mr. **Volkmer**. Now, should that be located at Brookhaven or where? Do you have any idea yet?

Dr. **Leiss**. I would like to answer the question in the following way. I think it is the view really of the Trilling Panel and HEPAP. They felt they would wait to see what would be the proposals for things that might fit within the budgets that they could see would be available given the guidance that is available. I think that Brookhaven would have a substantial head start with a lot of R. & D., and $120 million invested in the tunnel. I think that if they could not come through with the most competitive proposal, that would be telling us something.

Mr. **Volkmer**. All right. What are some of the most useful options for the ISABELLE complex at Brookhaven that would be less expensive than the original design?

Dr. **Leiss**. I think there are various things which are being considered and explored. One of these is a purely technical question that is being explored by Brookhaven right now. Can one put the coils of the—as you know, the ISABELLE is made up of two rings of magnets where the beams go in opposite directions. Can one put the coils of two of these magnets that are sitting side by side within the same iron yoke? There would be a substantial cost saving if that would work. They are exploring that possibility now.

The possibility exists that perhaps one would want to build as a first step an electron-proton colliding facility—which also is of very great scientific interest—and as a second step later on, as an upgrade, consider putting in a second proton ring.

There are a variety of options. There are some concepts called "missing magnets" in which you do not put in all of the magnets at one time. You put in part of them, and you get reduced energy which then could be later upgraded.

There are a variety of options which can result in lower cost even achieving a proton-proton colliding beam facility or variations of these.

I think that a specific feature that we can see is that essentially every option that would be interesting would involve at least one superconducting magnet ring in it. Therefore, the investment that is being made now in R. & D., we think, is well called for irregardless of how things turn out.

Mr. **Volkmer**. In other words, the R. & D. is not going to be lost in your estimation.

Dr. **Leiss**. Not in my estimation, sir.

Mr. **Volkmer**. All right. Now, the gentleman from New York has mentioned the investment that we have at Isabelle of $130 million approximately. How would we best protect this investment?

Dr. **Leiss**. It is obvious that the course we are trying to follow we believe is the most sensible and also prudent course for us to follow in trying to protect this investment and still use forward costs as wisely as possible.

Mr. **Volkmer**. In other words, to permit them to determine whether or not they can mass produce the magnets, etc. and also looking at the various options that may be available and that they are looking at for future utilization.

Dr. **Leiss**. And ways to get a cost within what we can see can be afforded given the budget guidance we have to deal with.

Mr. **Volkmer**. Which of the new generation of major facilities—ISABELLE, Tevatron I, Tevatron II, or the Stanford Linear Collider—has the potential to accommodate the largest number of experiments?

Dr. **Leiss**. I think there is no question, sir, that the fixed-target facilities which Tevatron II represents handles the largest number of users. However, I think that it is really very dangerous to look at it from that viewpoint because the scientific interest is also very important.

Among the colliding beam devices of that list—the Tevatron I, the Stanford Linear Collider and the Isabelle—there is no question that the ISABELLE facility is designed to handle a large number of users so it would certainly be the one.

Mr. **Volkmer**. In that regard, why is it so necessary to begin formal construction of the Stanford Linear Collider in the fiscal year 1984 such that the R. & D. phase can be completed in fiscal year 1986?

Dr. **Leiss**. The Stanford Linear Collider is an attempt to do R. & D. on a totally new concept of how to produce colliding beams of electrons and positrons at very high energy.

When you build a circulating beam colliding machine, the electrons radiate a great deal of energy in going around in a circle such that the LEP facility—the electron-positron colliding beam facility being built at CERN now—will have a total cost approaching $1 billion. That clearly is the end of the road in circulating beam electron-positron facilities. Therefore, if one wants to make progress in that direction in the future, one has to have a new invention.

The Stanford Linear Collider is such an invention. It takes beams of electrons and positrons out of the linear accelerator, bends them in an arc, and compresses them to dimensions of a few millionths of a meter and collides them with each other to get high luminosity. That is a whole new concept. It is something that has not been tried before. Before one could consider a really large facility for this, it is clear that one has to do a meaningful, yet still substantial, demonstration of this concept.

That is the reason why it is very interesting and important to do this R. & D. The timing is related to the fact that if this essentially R. & D. experiment is successful it will allow collisions between electrons and positrons at approximately 100 Gev center of mass energy. That should be enough energy to see the particles—the Z^o, W+, and W- particles—which are believed to be responsible for the coupling between the weak and the electromagnetic forces that have been predicted. It is a golden gleam in the eye of people to see this.

Therefore, the timing is based on a target of opportunity to observe these things. The fundamental motivation, in my opinion, for building the Stanford linear collider is to do the necessary research and development.

Dr. **KREBS**. Dr. Leiss, is it not probable that the Z^o and the W+, W-, will be discovered using European machines?

Dr. **LEISS**. It is certainly the hope at CERN that at least the Z^o will be discovered in the very near future with the proton-antiproton colliding beam capabilities that have been developed at CERN.

As you know, the luminosity that they have achieved so far is substantially below what they had hoped for, although it is coming up rapidly. They would hope to find the Z^o, maybe not the others, in that facility. Certainly the LEP facility has been designed to explore that energy region.

Dr. **KREBS**. But the SLC would not explore the energy region for the W+, W-?

Dr. **LEISS**. It depends a little bit on where the W's are. However, they would certainly hope to be above the threshold of the Z's.

Dr. **KREBS**. Thank you.

Mr. **VOLKMER**. We hear that the Tevatron I project at Fermilab would be rescoped?

Dr. **LEISS**. Yes, sir.

Mr. **VOLKMER**. How would this rescoping affect the project's total cost and completion date?

Dr. **LEISS**. We have not completed our rescoping activities but we have a pretty good idea that the cost of that facility is going to go up very substantially to slightly more than a factor of two over the original cost.

How that is going to affect the schedule is one of our toughest problems right now. When we try to figure out a TEC for our projects we have to have in mind a funding pattern and we have to fit that funding pattern into the overall program. Right now we are struggling with that question so we are not quite prepared to answer.

Mr. **VOLKMER**. That leads me to the question of whether the project will be able to utilize the $18 million that is requested in fiscal year 1983?

Dr. **LEISS**. We believe it will, sir.

It is the funding in the outyears, beyond 1983, that is our concern as to how to schedule that funding.

Mr. **VOLKMER**. I have a general question.

The gentleman from New York brought up the usage percentage at Brookhaven, and that people are basically waiting in line to use these facilities. Is this not true of all our user facilities at the labs?

Dr. **Leiss.** I believe that this is almost universally true of our research facilities. As long as it is not too bad, I think it is a very healthy thing because it forces the program committees in those laboratories to pick and choose what are the really important experiments.

I think with the reduced utilization of our facilities that has occurred in recent years, the waiting time on experiments has become too great. That is one of the prime reasons why we feel that improved utilization of our facilities is the priority we cannot avoid.

Mr. **Volkmer.** Do you think, Dr. Trivelpiece—you commented briefly—one of you did—on the industry's possible facility usage. I believe Dr. Kropschot commented on it with respect to the synchroton light source. What about the possibility of more industry cost sharing in the future? What are we looking at? Is there any possibility for this?

Dr. **Trivelpiece.** Certainly that facility, in its creation, had as a feature an up front commitment on the part of several industrial organizations to supply equipment and resources to bring it into existence. There was a commitment on their part to utilize it once it was brought into existence.

As I understand it, the equipment that is provided by the industrial users is to be made available on a one-fifth basis for users others than the industrial corporation that brought the equipment in. At the end of 3 years, ownership of the equipment that they supplied originally reverts to Brookhaven Laboratory for use by scientists in general.

I think it is a good example of how to go about creating facilities for component or multisegment users—industry, the laboratory, and universities. By having done it in this particular way we get the best advantages of industrial participation in a project that is very important.

Mr. **Volkmer.** Are the barriers to industry participation in the basic energy sciences program being removed, would you say?

Dr. **Trivelpiece.** They are certainly being reduced. There have been inhibiting factors that include aspects of the patent law which stem from the days of the Atomic Energy Commission. Those are being reduced as rapidly as possible and, for the most part except for some isolated cases I believe, industry can use these facilities provided they meet the tests of scientific appropriateness. Industry, if it is of proprietary interest to them—as Dr. Kropschot indicated—they do so on a fully cost-burdened basis. If it is of general scientific interest, they then agree to make that information available to the public.

Mr. **Volkmer.** The program that DOE has outlined here is an ambitious program in my opinion for basic energy sciences. They particularly advocate the systematic addition of at least one large modern facility—to enhance the capability of basic energy sciences programs. Is there a long-range plan by which these facilities are added as we look to the future? If not, do you think there should be one? How are we going to develop one?

Dr. **Trivelpiece.** I certainly could not be against long-range planning. Obviously, long-range planning is an essential ingredient of any successful scientific enterprise.

There is a certain degree which might be called anticipatory planning. That is that a collection of individuals who can see some distance into the future as they perceive it, identify what they believe to be—or will be—appropriate future needs and try to make some progress in trying to get commitments in that direction.

The other process which also takes place is that a particular area of science becomes recognized to a few individuals as being extremely important. They then propose that a facility to make it possible for them to make progress in that area be created. That proposal is then reviewed on a peer group evaluation basis and after some appropriate analysis, if it is agreed upon by the Department—or for that matter, other science-supporting agencies that might be interested in such things—then such facilities are funded and brought into existence.

The time for that is fairly long and the filtering process is rather elaborate before such facilities do come into existence. It takes, however, a combination of both. It takes a recognition of the spontaneous nature on the part of scientists that these are good things to do, together with what might be called more sober planning on the part of the departments and their advisory committees.

Mr. VOLKMER. Does the gentleman from New York have any additional questions?

Mr. CARNEY. If the high-energy physics community recognizes that ISABELLE is indeed capable—the project managers are capable—of producing the magnets and the project would go on, how much money would you anticipate that you would need from a budgetary standpoint on a yearly basis to keep it going to meet a target date, let us say, of 1987?

Dr. LEISS. I am confused, sir. Are you asking how much money would be required for the Isabelle project or for the high-energy physics program?

Mr. CARNEY. I guess that I am asking a hypothetical question. If it was found to be that ISABELLE—that the project—could be completed and that they have solved the last two problems and you went forward, how much money would be needed, on an annual basis, to continue the project? What would you need? $50 million per year or $60 million yearly?

Dr. LEISS. In the review that we did in October of this year—a rebaseline of the program—assuming that the project went forward in the fiscal year 1983 budget, we put together a schedule for the program based upon the plan and funding needs. The construction funding requirement varied from year to year but I believe averaged approximately $60 million per year. We could provide you with the schedule that was part of that plan.

Mr. CARNEY. Would you, please.

Dr. LEISS. It is not the operational plan at this time.

Mr. CARNEY. I would appreciate it if you would provide us with that schedule.

[Material to be supplied follows:]

BASELINE PLAN FOR ISABELLE
FROM DRAFT CONSTRUCTION PROJECT DATA SHEET SUBMITTED
BY BNL OCTOBER 26, 1981[1]

LINE ITEM CONSTRUCTION FUNDING[2]
(B/A in Millions of Actual Year Dollars)

PRIOR YEARS	FY 82	FY 83	FY 84	FY 85	FY 86	FY 87	TOTAL
$104.0	10.0	46.0	101.0	111.0	70.0	22.9	$464.9

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[1] Assumes cable magnet design and R&D funding for a first sextant magnet test.

[2] Includes allowances for contingency and escalation.

Mr. CARNEY. I would ask this question: Have you in your outyears budgeted moneys for ISABELLE in case it did jump the last two hurdles and you went forward with it? Have you provided for it?

Dr. LEISS. Within the budget, represented by the fiscal year 1983 budget and its projection in outyears, we have not provided money of the amount required to complete the present ISABELLE project. I think this is consistent with the report of the Trilling committee and the advice of HEPAP that within that budget level we simply cannot contain that project at its present cost without unreasonable damage to the remainder of the high-energy physics program.

Mr. CARNEY. In other words, you have no commitment at all to ISABELLE?

Dr. LEISS. That is correct, sir.

Mr. CARNEY. Why is that? If you are going to spend another $23 million for R. & D., why do you not have a commitment in out years? Have you already decided that they cannot jump the two hurdles of production?

Dr. LEISS. No, sir. We have asked them to try to solve their R. & D. problems and to try to find ways to reduce the cost of that facility, or an alternate facility——

Mr. CARNEY. But you have not provided any room for construction?

Dr. LEISS. Excuse me, sir. I said that we do not have room within that budget to contain a project of the price of ISABELLE. This was the conclusion of the Trilling Panel and HEPAP. The project that would be funded, with the budget guidance we now have, is not sufficient to pay for the construction costs of the present ISABELLE facility.

Mr. CARNEY. Then why are you spending $23 million in R. & D. if you are not going to fund it in the out years if they could get past their problems?

Dr. LEISS. We would like to have them come up with a facility at a price we can afford.

Mr. CARNEY. You had up there that you needed something in 1990. I am confused about this whole thing. Someone is kidding someone.

In other words, you have no intentions at all of continuing project ISABELLE even if they get past the last two hurdles that you speak of. Is that correct?

Dr. LEISS. We have reserved judgment on whether or what would be built as a facility. We want to see how they come on this R. & D. We want to see what cost savings they can achieve before we make final decisions.

Mr. CARNEY. But you have not provided for this in your fiscal year 1984–88 fiscal guidance scheme?

Dr. LEISS. That is correct, sir.

Mr. CARNEY. So what you are saying is that you are not funding it. You are not looking for them to solve the problems. Why are you spending the $23 million now for the R. & D.?

Dr. LEISS. I believe we are looking for them to solve the problem of how to build these magnets and how to come up with a proposal——

Mr. **Carney**. Well why are you doing that if you are not going to provide money in the out years to construct a project?

Dr. **Leiss**. We are hoping to be able to support a facility. It may not be as large and expensive as the present ISABELLE project, as defined.

Mr. **Carney**. What level of support, dollarwise, would you like to put into a facility and what have you provided in your 1984-88 fiscal guidance for a facility?

What are we going to do to answer that question about having something in 1990 or to meet your set of guidelines that were shown to us on the wall? How much of a fiscal commitment does DOE intend to commit to a new instrument in the out years?

Dr. **Leiss**. The conclusions were reached by the Trilling Panel but we have not taken a DOE position on this. I can only give you, at the present time, the advice of HEPAP and the Trilling Panel and that is that they felt that a sum of money on the order of $200 to $250 million was going to be available for the construction of a facility. If it were built at Brookhaven that would add on to the $120 million that has already been invested and that, sir, is a substantial sum of money.

Mr. **Carney**. Is that in the out-year budgeting that $200 to $250 million?

Dr. **Leiss**. That was in the out-year budgeting analysis done by the Trilling Panel.

Mr. **Carney**. Is it in your out year budgeting? Your fiscal guidance?

Dr. **Leiss**. We do not have that out-year budget planning available.

Mr. **Carney**. I do. Would you like to look at it? Would you like to take a look at it and tell me if you have it in there?

Dr. **Leiss**. I would have to look at the sheets you have, sir.

Mr. **Carney**. I will be glad to provide it for you.

Dr. **Leiss**. Perhaps Mr. Mayhew can answer for me so that I do not say it wrong.

Mr. **Mayhew**. Mr. Carney, I believe the numbers that we are looking at in the table you just provided—the out-year numbers—represent simply an extension of those activities underway in fiscal year 1983. To the extent that there is no line item construction funding for ISABELLE in 1983, I believe that it is fair to say there would not be funding for the ISABELLE line item in those out-year numbers.

Mr. **Carney**. It is fair to say that you have absolutely no money for ISABELLE in the out-years—fiscal year 1984-88. I would have to assume that you have made a commitment already that no matter what effort they make with their $23 million, ISABELLE has been viewed very prejudicially from the standpoint of DOE. I think that would be fair to say.

Dr. **Leiss**. It is clear that we do not agree with you, sir, on that.

Mr. **Carney**. It is clear that there is no money in that schedule.

Mr. **Mayhew**. The document that we are looking at represents an internal guidance document. It is just now being discussed within the Department. It reflects a base number provided by OMB for planning purposes for 1984 through fiscal year 1988. The fiscal year 1984 numbers would include provision for an extension of the

R. & D. funding on Isabelle underway in fiscal year 1983. If one studies this document that you have provided, I believe it also indicates that there are opportunities to make a case for, and try to justify, amounts above that fiscal year 1984 base number. That is simply a number from which they will try to track changes, both up and down.

Mr. VOLKMER. Will the gentleman yield?

Mr. CARNEY. I will be glad to yield to the chairman.

Mr. VOLKMER. I would just like to question Dr. Trivelpiece now that he is back—and it is along this line.

We see a substantial increase in percentages anyway, not necessarily in dollars but in percentages, in the basic research field in the budget for 1983. That is a discretionary funding program. Almost all other discretionary funding programs, not only in DOE but all across the Government, are getting reduced generally.

The question that I have is, in the future do you see this continued rise in the same amounts for our basic research with the budgetary constraints? As a discretionary budget, the total amount of funds in there strikes me as being reduced. Do you think you will be able to continue to have the increases? I am talking about outyears.

Mr. TRIVELPIECE. Mr. Chairman, I do not know what transpired in my absence but the administration has tried to make a strong commitment to certain areas of basic research and technology-base activities. I think that the budget presented here today reflects that commitment. These increases were not, when looked at—in the case of the high-energy physics program—over a 2-year span, really do not represent a large percentage increase. I think, in fact, that the average over 2 years is in the order of 7 or 8 percent which is probably barely equal to inflation. And, certainly in the basic energy sciences program, as Dr. Kropschot indicated, part of the growth is apparent in that two programs were transferred into the activity and they brought their budgets with them. The remainder is primarily reflecting a constant level of effort—of activity. They have been singled out for special treatment by the administration but they certainly have not been allowed to have very large growth.

Mr. VOLKMER. From some figures that I have, I estimate that since 1981, the budget compared to 1981 and 1983, we still have an increase in the energy research programs—the total programs—of between 16 and 20 percent.

Dr. TRIVELPIECE. I am not sure what you are including in the basic energy research program, but the high-energy physics program over the 2 years is something like 8 percent per year.

Mr. VOLKMER. Is that in real dollars or in inflated dollars—total dollars? The total dollars that I have in high-energy physics between those years is that we have a 22-percent total in the 2-year period. My figures may be wrong but those are the figures that I have. In nuclear physics we only have about a 5-percent increase, but I have another question on that in a minute.

Dr. TRIVELPIECE. Nuclear physics, as part of the high-energy nuclear physics programs, did not have as large a growth as the high-energy physics part. In fact, when increased prices and so on are taken into account, there is a slight decrease in the nuclear physics.

Mr. **Volkmer**. That is the next question really. How will the reduction in heavy ion nuclear physics' operating expenses impact research at the major facilities—the national laboratories such as Lawrence Berkeley, Oak Ridge, Brookhaven, Argonne?

Dr. **Trivelpiece**. It will certainly have some impact on them. Perhaps Dr. Leiss would like to answer in more detail?

Dr. **Leiss**. Sir, it will have impact. We will have to reduce the operation of our facilities some compared to what we would like to do.

I think I have enumerated the impacts that have occurred at the double MP tandem at Brookhaven. Similar reductions in operations on the Super-Hilac at Berkeley—perhaps somewhat more severe reductions on the operation of that facility will have to occur as part of the program.

Mr. **Volkmer**. Will we see a continued reduction in the heavy ion activity next year? Also, do you anticipate that in 1984? In 1985?

Dr. **Leiss**. I certainly would hope not, sir. We will try to avoid that if we can.

Mr. **Volkmer**. Is there the same importance, more importance, or what, as far as research in heavy ions is concerned as against medium energy research?

Dr. **Leiss**. I think in putting the program together, within the budget guidance we were given for the nuclear physics program, we had to first look at our facilities. We had two major facilities, the Los Alamos Meson factory and the Bates Linear Accelerator, which are quite unique worldwide facilities in their capabilities.

We felt that the degree to which we could reduce the support of those facilities and their users was limited. After that we had to look at the rest of the program. In other words, we provided a certain degree of protection of these major, very expensive facilities at Los Alamos and at MIT.

Mr. **Volkmer**. What utilization rate would you be providing for those two approximately?

Dr. **Leiss**. In fiscal year 1983—I have the numbers here—the Los Alamos Meson factory is projected to operate approximately 2,500 hours against an estimated operation of 3,100 hours in fiscal year 1982.

Mr. **Volkmer**. What about the MIT?

Dr. **Leiss**. The MIT machine is projected to operate 3,000 hours against 2,500 hours in fiscal year 1982. I would, however, remind you, sir, that a major construction was going on related to the upgrade of that facility, and the 3,000 hours is an attempt to recover on our investment in using the facility.

We felt it would be very dangerous indeed to reduce these operations below those levels.

Mr. **Volkmer**. In other words, you are saying that approximately 3,000 or 3,100 hours is what the facility needs to be utilized?

Dr. **Leiss**. No, sir. I am not saying that at all.

I am saying that LAMPF operated 3,100 hours in 1982 and it is projected to run 2,500 hours in 1983. We believe that if the funding were available, we would like to operate that facility on the order of 4,000 hours or more. We feel that it would be—that 2,500 hours is as low as we should allow that operation to go.

Those were considerations that went into deciding how the budget was put together.

Mr. **Volkmer**. Does the gentleman have any further questions?

Mr. **Carney**. No, Mr. Chairman, I do not.

I would just like to note for those in the audience, and particularly since this is the Science and Technology Committee, that Columbia III is safe and on the ground in New Mexico.

I would also like to note that when you have a commitment to science, you can do those things.

Mr. **Volkmer**. I wish to thank Dr. Trivelpiece, Dr. Kropschot, and Dr. Leiss.

That will conclude the hearings. I appreciate your testimony here today. Thank you very much.

[Whereupon the subcommittee adjourned at 11:20 a.m.]

[Additional material submitted for the record follows:]

Department of Energy
Washington, D.C. 20585

April 26, 1982

Honorable Don Fuqua
Chairman, Subcommittee on Energy
  Development and Applications
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

Subsequent to the March 30, 1982, hearing before the House Science and Technology Subcommittee on Energy Development and Applications, questions were submitted by members of the Subcommittee. Answers to these questions are enclosed.

Also, copies of the answers have been provided to the Subcommittee to complete the official record.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas H. Pretorius

for Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

cc: Ms. Mary Bly
    Subcommittee on Energy Development
      and Applications

High Energy Physics - General Questions

1. Are the DOE and NSF high energy physics programs truly complementary? What do you see as the future roles of NSF and DOE, respectively, in high energy physics research?

The DOE and NSF programs are indeed complementary. The DOE program is focussed on operation and provision of facilities at the three major U.S. high energy physics centers and also includes substantial support of university research groups. The NSF program focusses on support of university groups and also includes support of the Cornell Electron Storage Ring (CESR) facility. Each of the U.S. high energy physics facilities is available to qualified users on the basis of the scientific merit of their proposals on a competitive basis and thus many of the NSF-supported groups are users at DOE-supported facilities and DOE-supported groups are users at Cornell. Because DOE provides over 90 percent of the Federal support of the U.S. program, DOE is responsible for serving as the lead agency with responsibility for planning the national program. The U.S. high energy physics program has been highly successful over the decades because of this joint government agency support effort involving DOE and NSF participation. We see no reason to suggest any changes in the present roles.

High Energy Physics - General Questions

2. The high energy physics community has ambitious plans for new, large and expensive research facilities, but we are faced with indefinite and severe budget constraints. What plans are you making for international cooperation in building and operating large facilities in the future?

There have been numerous DOE discussions on international cooperation in the construction, management and operation of large high energy physics facilities for many years in recognition of the fact that the very large facilities required near the end of the century may be so costly that no single region could afford to build its own facility. There is currently an International Committee for Future Accelerators (ICFA) with representatives from the U.S., Japan, Western Europe, and the USSR which continues to consider these matters. There are many complex management, logistical, funding and international relations matters which must be worked out before a joint venture of such magnitude would have reasonable probability of success. While we do not see a joint international venture as feasible or necessary for the facilities of this decade, we recognize the importance of planning for such ventures for the more distant future. ICFA is in the process of setting up two task forces, one to make studies on a 20 TeV fixed target proton accelerator and the second on an electron-positron collider of about 300 GeV energy per beam. The possibility of joint participation in such future major accelerator facilities, when they pass the tests of research significance and cost effectiveness, is being considered by the U.S., especially in concert with Western Europe and Japan. We are pleased that ICFA is laying the ground rules.

High Energy Physics - General Questions

3. In your view does the FY 1983 request for high energy physics reflect a proper balance between physics research, facility operations, facility construction and long range R&D for the future generations of facilities?

We believe that our FY 1983 request does provide a well-balanced high energy physics program. The budget provides for a recovery from the very low utilization levels of recent years, effective support for university research groups, continued construction progress on the Tevatron I and Tevatron II facilities to provide 1000 GeV X 1000 GeV proton-antiproton colliding beam and 1000 GeV fixed target capability at Fermilab, a strong R&D effort at BNL to demonstrate the performance of the new cable superconducting magnet design and to develop and demonstrate cost effective production techniques and R&D effort at Stanford Linear Accelerator Center (SLAC) on a new concept for very high energy electron-positron colliders. The program thus includes effective and reliable operation of present facilities, effective support of research scientists, continued progress on Tevatron construction projects and two major R&D efforts related to possible future facilities.

Basic Energy Sciences

1. How do you see the difference in roles between the universities and laboratories in the Basic Energy Sciences Program?

BES is a strong sponsor of university research and since 1975 has steadily increased this fraction of its research program. Support to universities is provided in several ways. Sponsorship of research at university based institutions, Lawrence Berkeley Laboratory and Ames Research Laboratory in Iowa, which are also National Laboratories, is one example. A second example is allocation of substantial blocks of funding to institutions, which then have considerable discretion and flexibility in management of the research. The University of Illinois Material Science Center, the Courant Institute at New York University and the Plant Sciences Laboratory at Michigan State University are examples. The largest part of the university support, however, is provided through special research contracts with individual universities. BES currently has more than 600 of these at over 150 colleges and universities. Awards are made after careful peer review by experts in the particular field of research. We value these university contributions most highly. Not only does this sector produce some of our best science, it also produces a tremendous bonus: graduate students trained in the energy fields.

Research supported at the national laboratories falls into several categories. Their most important function is to maintain a first class, productive scientific and engineering staff that is dedicated to energy research. There are advantages to the national laboratories that are not always understood:

- They build and operate large, often unique frontier research facilities for general scientific use.
- Their proximity to the more applied work done at the laboratories is often crucial in understanding problem areas and priorities.

- They are able to undertake large, interdisciplinary projects that are impractical to break down into many "thesis-sized" components.
- They do some kinds of essential research that are impossible or inappropriate for universities.

Over 40% of the funds provided to the national laboratories are used to operate and conduct research associated with large facilities such as the nuclear reactors, National Synchrotron Light Source, electron microscopes, pulsed neutron sources, and the Combustion Research Facility. The balance of the funding supports the conduct of interdisciplinary or non-thesis programmatic research which in general is not appropriate for universities, including much of the acquisition, compilation and critical evaluation of data, and maintains the laboratories' scientific capabilities.

Basic Energy Sciences

2. There are several smaller programs in basic energy sciences; for example, in geosciences, engineering, and biological energy. What do you think are the ultimate appropriate levels for these efforts and do you think that they should be funded at the expense of the more established programs in materials research, chemistry, or nuclear sciences?

In our budget request and long-range planning, high priority continues to be given to increasing our support of research related to energy problems in geosciences, engineering and biological sciences. This comes about because our reviews show major research needs in these areas, and our interactions with these scientific communities indicate significant opportunities worth pursuing. We have long range plans for the next five years that would require a level of about $35 million per year for geosciences research, including DOE participation in a major effort in Continental Scientific Drilling called for in a series of National Academy of Sciences studies; of $30-$50 million in engineering research to assure the needed technology base; and of $20-$25 million in the promising field of biological energy research. However, increases of this magnitude cannot be made at the expense of more established programs without seriously eroding high priority research in materials, chemical and nuclear sciences where DOE plays a critical national role. The growth of the newer programs would round out our national ability to explore the vast array of possible concepts and options for meeting our longer term energy needs. At the same time, it is in the more mature, core programs that major new facility growth, in synchrotron radiation research, pulsed neutron activities, ion-laser instrumentation, combustion research and electron microscopy, among others, has resulted in important new advances applicable in a number of areas, including biological structure. We will, however, be alert to important new developments, particularly in biological research, and be prepared to reprogram sufficient funds to support unique new research or to provide a foundation for future growth.

Basic Energy Sciences

3. There are several areas for which DOE's Basic Energy Sciences and NSF interface and overlap. Is this overlap desirable? Are the respective DOE and NSF subprograms well coordinated? What do you see as the future roles of NSF and DOE, respectively, in basic research?

Appropriate relationships and complementary overlap do exist which permit and encourage the information and concept exchange necessary to stimulate scientific advances. Plurality of research support encourages diversity and is healthy. As stated by Sir Fred Hoyle the eminent British astrophysicist, it is this diversity and plurality of support that, in the past, made U.S. science different from the British and Soviet science and resulted in enhanced U.S. superiority.

The Basic Energy Sciences program interfaces effectively with several NSF programs in the physical, biological and engineering sciences. Coordination occurs at various levels including interagency working groups, open review of laboratory programs, meetings of program managers, publication of project listings, and at the most important and effective level of the principal investigators.

While DOE supports basic research that also may be appropriate to NSF, our reasons are guided by our mission. The research supported by BES is selected within the framework of a coherent program constructed with an awareness of energy technology base needs and research opportunities. It is directed towards the long range aspects of energy, its production from all sources and its efficient and environmentally sound use. Thus, for example, our support of catalysis research under several of our programs reflects the important role fundamental knowledge in this area plays in energy conversion, conservation, energy storage, and fossil energy systems.

Basic Energy Sciences
3. continued

Also, the DOE Laboratory system has traditionally provided the lead in establishing major facilities for research; such facilities, requested by DOE to meet DOE needs, frequently become major national facilities available for U.S. scientists supported by other agencies and by industry.

I expect DOE to continue its traditional role for support of basic research with regard to both major facilities and its energy mission. I look forward to NSF's continued support of basic research independent of particular technology base needs, strongly supportive of the training of future generations of American scientists.

Basic Energy Physics

4. DOE's Basic Energy Sciences subprograms are currently organized according to scientific disciplines. Is there any merit to organizing them according to interdisciplinary problem areas?

The most important aspect of our program in Basic Energy Sciences is to use and develop the national scientific capability which in turn will provide the technology base for solving energy problems. Basic research starts from the fundamental sciences and the knowledge, understanding and application follows from the research. The capabilities for conducting research within both the universities and the national laboratories are organized primarily by discipline in departments or divisions of chemistry, physics, material science, biology, etc. We do have interdisciplinary and phenomena oriented research currently underway in BES on topics such as combustion and catalysis but it is based in the disciplines. Managerially, we have the flexibility and are able to focus activities under the several disciplines toward interdisciplinary areas, e.g., the integration of materials, geosciences and chemistry research projects focused on long term nuclear waste isolation problems. Thus, I see no compelling reason to reorganize by problem area since problem areas change more frequently than the disciplines used to solve these problems.

Nuclear Physics

1. Has DOE considered limiting the scope of the DOE nuclear physics program? If so, how?

The scope of each Office of Energy Research program, including Nuclear Physics, is determined by a balance of scientific and technical priorities against budgetary constraints. We would like to say that the scope of each Energy Research program is limited only by the imagination of its scientists. However, in the real world, this is just not possible. In formulation of the FY 1983 request for Nuclear Physics, the very real needs of the Nuclear Physics program had to be weighed against the very real needs of other programs within a budget total constrained by national policy. Even though the FY 1983 budget request will require a small reduction in the overall DOE Nuclear Physics effort below FY 1982 levels of activity, we are convinced that a scientifically productive effort in basic nuclear research in FY 1983 will be possible.

In this connection, it is important to recognize that NSF also supports an appreciable fraction of the U.S. effort in Nuclear Physics. In FY 1983, the President's request for the NSF program contains an increase of about $7.7 million (60%) above the FY 1982 level. Most of this increase is associated with the transfer of responsibility for the construction of the Michigan State Project - Phase II from DOE to NSF.

Nuclear Physics

2. Do you favor de-emphasizing the "nuclear structure" component of the program?

Nuclear structure research will continue to be an important scientific component of the overall Nuclear Physics research effort. For example, we are very much interested in understanding the role of quarks and supersymmetries in the structure of nuclei. However, at the present time, the greatest scientific opportunities appear to be in nonstructure areas such as in studies of the mechanisms by which mesons, heavy ions, protons, and other probes interact with nuclei; in studies of the behavior of nuclear matter under extremes of excitation energy and angular momentum; in studies of the mechanisms by which colliding nuclei exchange mass, energy, and angular momentum; and in studies of the fundamental strong, weak, and electromagnetic forces which determine the structure and behavior of nuclei.

Nuclear Physics

3. The Brookhaven Double Tandem facility has a large demand from users and has great potential for upgrade in the near future. How can DOE's proposal to initiate phase out of this facility in FY 1983 be justified?

In formulation of the FY 1983 budget for Nuclear Physics, highest priority was given to the internationally unique scientific capabilities of the Clinton P. Anderson Meson Physics Facility at Los Alamos (LAMPF), the William H. Bates Electron Linear Accelerator at Massachusetts Institute of Technology, and the Lawrence Berkeley Laboratory Bevalac. A very high priority also had to be assigned to the Argonne Tandem/Linac Accelerator System (ATLAS) project which is now under construction and the Holifield Heavy Ion Research Facility at Oak Ridge which began its research program in 1981. ATLAS and Holifield have or will have beam capabilities superior to those now available at the Brookhaven Double Tandem Facility. ATLAS will be and Holifield is operated as a national facility open to all qualified American scientists. These two facilities and the Michigan State Superconducting Cyclotron project, which is being transferred to the National Science Foundation, will be the leading research facilities for low energy, heavy ion nuclear research in the 1980's. The low energy, heavy ion beams of the Brookhaven Double Tandem are certainly needed until at least FY 1986 at which time ATLAS is scheduled for its first beam. However, within a constrained overall budget for Nuclear Physics, difficult decisions such as initiation of phase-out of the Brookhaven Double Tandem had to be made.

Brookhaven has proposed upgrade of the Double MP Tandem Facility by addition of a cyclotron to the existing accelerator complex. This proposal is now under review by the DOE/NSF Nuclear Science Advisory Committee (NSAC) in competition with six other proposals for upgrade of nuclear research facilities. In its

Nuclear Physics
3. continued

review, NSAC must evaluate the proposed Brookhaven upgrade as one component of a balanced national program for basic nuclear research. A decision to upgrade the Brookhaven facility would certainly increase the priority assigned to the Double MP Tandem Facility within our planning for Nuclear Physics.

Nuclear Physics

4. Are the DOE and NSF nuclear physics programs complementary? Is some overlap desirable?

I am convinced that the answer to both questions is "yes." In order to conduct experiments at the forefront of science, basic nuclear research scientists require the existence and effective operation of large national facilities. These unique facilities are made available to all American scientists on the basis of scientific merit. Because of their size and complexity, some of these facilities are best sited at national laboratories or large, single purpose laboratories such as the William H. Bates Electron Linear Accelerator at Massachusetts Institute of Technology. In addition to the national user facilities supported by DOE, NSF supports major facilities at Indiana University and Michigan State University. The Department of Energy's Nuclear Physics program emphasizes, but is not limited to, construction, management, and research at these unique, forefront facilities for advanced experiments. Whereas, the National Science Foundation emphasizes, but is not limited to, support of the smaller on-campus, university-based facilities whose programs have a major education and training component. Both areas of emphasis are essential to a balanced national program of basic nuclear research.

Because nuclear research does not and should not separate neatly into Department of Energy and National Science Foundation areas of emphasis, some overlap is desirable. Our principal concern is adequate coordination of agency activities rather than the existence of overlap. We consider this complementary approach very healthy and one of the strengths of the United States approach to support of basic nuclear research. Through the efforts of the DOE/NSF Nuclear Science Advisory Committee and frequent interaction of National Science Foundation and Department of Energy staffs, we believe that adequate coordination does exist.

Nuclear Physics
4. continued

4. What do you see as the future roles of NSF and DOE, respectively, in nuclear physics research?

We believe that the present areas of emphasis are appropriate to the capabilities of the two agencies. The strength of the U.S. program in nuclear physics is a clear indication that the present arrangement is working well. However, new directions for each agency must be developed carefully and with adequate input from the scientific community. An important source of scientific input is the DOE/NSF Nuclear Science Advisory Committee which has developed an excellent reputation for the quality of its recommendations.

5. Is there currently duplication among nuclear physics research facility capabilities? Is some duplication desirable?

In terms of low energy, heavy ion beams and low energy, light ion beams available at electrostatic accelerator and cyclotron facilities, there is some duplication. However, this kind of duplication is necessary to meet the research demand for such beams. A single facility could not satisfy the experimenters' demand for beam time. In terms of scientific experiments, there is little or no duplication. A crucial or startling scientific result must be examined. This is almost always done by devising new experiments to provide new insight into the phenomena under investigation rather than duplication of a previously executed experiment.

On a continuing basis, Department of Energy staff, National Science Foundation staff, and the DOE/NSF Nuclear Science Advisory Committee (NSAC) examine the number and mix of nuclear research facilities in the United States to assure that present and projected research requirements are met without unnecessary duplication. NSAC has recently completed a study of the facility requirements for low energy, heavy ion research and is near completion of study of the facility requirements for nuclear research with electromagnetic probes.

6. Is the shutdown of some university-based nuclear physics facilities justified?

When a university-based or national laboratory-based facility ceases to be sufficiently productive to warrant continued Federal support, then shutdown is not only justified but is also necessary for the overall vigor of basic nuclear research in the United States. Without periodic upgrade or replacement, any scientific facility can be expected to eventually reach the end of its useful scientific life. However, the more common situation is one of relative priorities within a given budgetary situation. The highest priority facilities, university-based or national laboratory-based, must be effectively operated. Sometimes this means shutdown of lower priority facilities even though they have not reached the end of their scientific lives.

Nuclear Physics

7. What are the prospects for greater international cooperation in nuclear physics facility construction and operation?

On a reciprocal and equitable basis, American, Canadian, Japanese, and Western European scientists have access to the major accelerator and reactor research facilities in the United States, Canada, Japan, and Western Europe. The exchange is open, fair, and generally conducted on a scientist-to-scientist or laboratory-to-laboratory basis rather than government-to-government basis. For the current scale of nuclear research facilities, the present system of national operation and construction of facilities works well. As the need for larger nuclear research facilities evolves, it is quite likely that international construction and operation will be the preferred way to proceed. In a very preliminary and informal fashion, American, West European, and Japanese scientists have discussed the possibility of an international facility for very high energy, heavy ion nuclear research.

[illegible]

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[illegible]

April 1, 1982

Dr. Alvin W. Trivelpiece
Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

Thank you for the testimony which you presented on the Fiscal Year 1983 budget for the Department of Energy's programs in Supporting Research and Technical Analysis, and in High Energy and Nuclear Physics.

During the hearing you indicated your willingness to respond to written questions from Members of the Subcommittee. Enclosed are a number of questions to which we would appreciate receiving responses.

Sincerely,

Don

DON FUQUA
Chairman

DF:cw
enclosure

Questions
Hearing on FY 1983 DOE Authorization for Basic Research Program
March 30, 1982

Questions on High Energy Physics

General Questions

1. Are the DOE and NSF high energy physics programs truly complementary? What do you see as the future roles of NSF and DOE, respectively, in high energy physics research?

2. The high energy physics community has ambitious plans for new, large and expensive research facilities, but we are faced with indefinite and severe budget constraints. What plans are you making for international cooperation in building and operating large facilities in the future?

3. In your view does the FY 1983 DOE request for high energy physics reflect a proper balance between physics research, facility operations, facility construction, and long range R&D for the future generation of facilities?

Questions on Basic Energy Sciences

1. How do you see the difference in roles between the universities and laboratories in the Basic Energy Sciences Program?

2. There are several smaller programs in basic energy sciences; for example, in geosciences, engineering, and biological energy. What do you think are the ultimate appropriate levels for these efforts and do you think that they should be funded at the expense of the more established programs in materials research, chemistry, or nuclear science?

3. There are several areas for which DOE's Basic Energy Sciences and NSF interface and overlap. Is this overlap desirable? Are the respective DOE and NSF subprograms well coordinated? What do you see as the future roles of NSF and DOE, respectively, in basic research?

4. DOE's Basic Energy Sciences subprograms are currently organized according to scientific disciplines. Is there any merit to organizing them according to interdisciplinary problem areas?

Questions on Nuclear Physics

1. Has DOE considered limiting the scope of the DOE nuclear physics program? If so, how?

2. Do you favor de-emphasizing the "nuclear structure" component of the program?

3. The Brookhaven Double Tandem facility has a large demand from users and has great potential for upgrade in the near future. How can DOE's proposal to initiate phase out of this facility in FY 1983 justified?

4. Are the DOE and NSF nuclear physics programs complementary? Is some overlap desirable?

What do you see as the future roles of NSF and DOE, respectively, in nuclear physics research?

5. Is there currently duplication among nuclear physics research facility capabilities? Is some duplication desirable?

6. Is the shutdown of some university-based nuclear physics facilities justified?

7. What are the prospects for greater international cooperation in nuclear physics facility construction and operation?

REPORT OF THE SUBPANEL ON LONG RANGE PLANNING FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM OF THE HIGH ENERGY PHYSICS ADVISORY PANEL

January 1982

U.S. Department of Energy
Office of Energy Research
Division of High Energy Physics
Washington, D.C. 20545

Department of Energy
Washington, D.C. 20545

HIGH ENERGY PHYSICS ADVISORY PANEL

February 26, 1982

Dr. Alvin Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, D. C. 20585

Dear Al,

I am transmitting to you the Final Report of the HEPAP Subpanel on Long Range Planning for the U.S. High Energy Physics Program, chaired by Dr. George Trilling of U.C.-Berkeley. HEPAP discussed this report at its meeting at DOE Headquarters in Germantown, Maryland, on February 19-20, 1982, and supports its recommendations. This Subpanel report completes the most extensive study of the national High Energy Physics Program of recent years. It presents the Department of Energy with a valuable analysis of the scientific prospects and of the technical status and opportunities; and, on behalf of HEPAP, I wish to thank the chairman, Dr. Trilling and the Subpanel members for their arduous and important labors.

As stated by the Subpanel: "The future of high energy physics promises to be as exciting and productive as its profoundly successful recent past. Whereas the U.S. program has dominated this field until recently, the importance of high energy physics has stimulated vigorous competition from other regions of the world." Looking ahead at the 1980's and into the 1990's and facing the challenge of maintaining a vital and eminent program, the strategic question in U.S. high energy physics is the balance between utilization of existing facilities, the construction of the major facilities for the next stage of research, and the advanced accelerator research and development effort necessary for the long-range progress of the field. The problems of formulating a successful policy are difficult and complex, and the three major program elements mentioned above are often coupled in the consideration of specific projects.

The Subpanel has arrived at a set of recommendations which it believes will provide the necessary ingredients for a long-term vital and competitive U.S. program. This program requires an annual support which rises to about $440M DOE and $35M NSF (FY 1982 dollars) ($430M and $38M respectively in FY 1983 dollars) by 1984 or 1985 at the latest.

Its key elements are:

Construction of ISABELLE at Brookhaven with completion by the end of this decade.

Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

Expeditious completion of the Energy Saver at Fermilab.

Implementation of the Tevatron II (TeV II) fixed-target program at Fermilab, in all three experimental areas.

Completion of the Tevatron I (TeV I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

HEPAP strongly endorses this recommendation as providing the essential elements for a healthy, albeit lean U.S. program in high energy physics. It provides for a level of effort about the same as the 1978 DOE/OMB Agreement and permits effective utilization of present facilities, construction of the new facilities required by the end of the decade, and advanced technology R&D essential for the program future. Although well below the support level of the Western European program the level of the DOE/OMB Agreement would still allow us to compete effectively on a world basis. HEPAP urges that the annual budgets for high energy physics be restored to the level required for this balanced program.

To respond to the eventuality that the above level is not reached in the near future, the Subpanel carefully studied the program at a continued budget level equal to $395/34M in FY 1982 dollars. This is close to the level in the President's proposed FY 1983 budget ($429M for DOE). It is about 9 percent below the budget level of the 1978 Agreement between OMB and DOE which identified the minimum level under which the construction of ISABELLE and of the Tevatron project could be initiated. At the $395/34M level the budgetary impact on the other essential elements of the program of continuing the present ISABELLE project is severe. If the budget remains no higher than this level, the Subpanel recommends and HEPAP concurs that this construction project cannot continue.

The Subpanel concluded that it is essential for an active and healthy U.S. program in the early 1990's that a major new facility for experimentation be available by the end of this decade. If the present ISABELLE project must be discontinued, then another major facility must be started in the mid-1980's so as to be available for research by 1990. This facility would have to be less expensive than ISABELLE but must be capable of supporting a broad physics program and of exploring new frontiers. HEPAP strongly agrees that such a new facility is essential by the end of the decade. By that time most of the present facilities will be past their peak usefulness and unable to meet the requirements of many forefront experiments. Thus a major new project is clearly required if the facilities available to U.S. high energy physicists in the early 1990's are to be competitive and adequate to the intrinsic potential of the field.

The Subpanel has recommended that the linear collider project SLC, the major R&D effort at SLAC, proceed so that the R&D phase, including construction of necessary conventional facilities, can be completed in 1986. HEPAP strongly endorses this recommendation. The SLC was discussed at length and HEPAP wishes to comment here briefly on its significance to the U.S. program. The SLC is an accelerator research project which is central to the long term development of a very high energy electron-positron collider, and at the same time offers the opportunity to do some very exciting physics in a timely fashion. It is this latter point which drives the schedule. The SLC is not the new major facility for the 1990's recommended by the Subpanel and endorsed by HEPAP; it is an example of the type of relatively large R&D project which is now necessary in the development of new accelerator systems. Viewed in the light of the cost, complexity, and scientific importance of a future very high energy linear collider, the SLC is a reasonably scaled construction effort for R&D purposes, and is strongly supported by HEPAP.

HEPAP strongly emphasizes the importance of maintaining the budget at a level at least equal to that currently requested for FY 1983 ($429M for DOE). This budget level will permit important improvement in FY 1983 of the currently poor utilization and inadequate support of existing facilities, and also, expeditious completion of the Fermilab Energy Saver. HEPAP concurs with the Subpanel assignment of highest immediate priority to these two program elements. At this budget level the U.S. program will be competitive and strong through the 1980's. However, HEPAP also reemphasizes the importance of a major new facility so that we can envisage a strong and vigorous national high energy program in the 1990's.

Sincerely,

Sid

Sidney D. Drell
Chairman, HEPAP

50th
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Lawrence Berkeley Laboratory
University of California • Berkeley, California 94720

February 10, 1982

Professor Sidney Drell, Chairman
High Energy Physics Advisory Panel
Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, CA 94305

Dear Sid:

I am submitting herewith the Final Report of the 1981 - 1982 HEPAP Subpanel on Long Range Planning in High Energy Physics. I sincerely hope that this Report will be of some value to HEPAP, to the Department of Energy, and to the National Science Foundation as they chart the future course of the national high energy physics program.

Sincerely,

George H. Trilling

George H. Trilling

Enclosure

GHT:cfd

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SECTION I

PREFACE

The High Energy Physics Advisory Panel (HEPAP) Subpanel on Long Range Planning was formed in August 1981 to advise on the U.S. High Energy Physics Program. The Charge to the Subpanel is given in Appendix A, and the membership is listed in Appendix B.

The Subpanel held an initial organizational meeting on September 4-5, 1981, in Washington, D.C. On September 24-26, 1981, the Subpanel met at DOE Headquarters in Germantown, Maryland, and heard presentations from representatives of the high energy accelerator laboratories--Cornell, SLAC, Fermilab, and Brookhaven--and from various other individuals. The Subpanel has since held further meetings on October 9-10, 1981, at Brookhaven, on October 17-19, 1981, at DOE Headquarters in Germantown, Maryland, on December 2-5, 1981 in Napa, California and on January 14-17, 1982, at NSF Headquarters in Washington, D.C. A Technical Subgroup of the Subpanel supplemented with various consultants made further visits to Fermilab (October 1-2); to Brookhaven (October 8-9), to SLAC (November 3-4) and to Cornell (November 19-20) to assess the technical status as well as the anticipated schedule and costs for the Tevatron I (TeV I) project at Fermilab, the ISABELLE project at Brookhaven, the Stanford Linear Collider (SLC) project at SLAC, and the CESR II project at Cornell. The list of Subpanel members and consultants who participated in these visits is given in Appendix B. To solicit input from the high energy community, a letter from the Subpanel Chairman inviting such input was sent to each member of the Division of Particles and Fields of the American Physical Society. About 170 responses to this invitation, representing the views of 240 people, were received and distributed to members of the Subpanel.

The present Subpanel is the fifth in a series of recent Subpanels (1974, 1975, 1977, 1980) which have reviewed the status of the U.S. High Energy Physics Program and made recommendations for future directions. Both the ISABELLE project and the colliding beam effort at Fermilab were recommended by the 1977 Subpanel. The anticipated funding level on which these recommendations were based was higher than the highest level of the present Charge; namely, 10 percent above the DOE/OMB Agreement of 1978. However, the 1977 Subpanel did consider a lower level, closely equivalent to the intermediate level of the present Charge (the DOE/OMB Agreement of 1978), still recommended the ISABELLE project, but stated that "this funding level on a long term basis is close to that which would require a drastic revision in the long range program and probably is not adequate to keep the U.S. in a world competitive position in this field."

As requested by the Charge, an Interim Report was submitted on November 1, 1981, evaluating the ISABELLE priority for various constraint levels. The basic conclusion of the Interim Report can be summarized by repeating here the recommendation given therein:

> "The Subpanel strongly recommends that the ISABELLE project go forward and be completed during this decade. ISABELLE provides a major new facility that will enable the U.S. High Energy Physics Program to remain

active and healthy during this decade and into the next. ISABELLE construction and the other essential components of the U.S. Program require a minimum annual level of support that averages $440M (FY 1982 dollars) in DOE funds per year. If support at this level cannot be made available in time for ISABELLE completion within this decade, the ISABELLE project cannot be continued."

The Interim Report was presented to HEPAP at a meeting held November 1, 1981, in Washington, D.C. and was unanimously endorsed.

The present document is the Final Report of the Subpanel. In its long-range planning recommendations, the Subpanel has considered in detail two specific yearly budget levels; namely, (1) $440M DOE and $35M NSF, and (2) $395M DOE and $34M NSF (FY 1982 dollars). These are respectively 4 percent above and 8 percent below the DOE/OMB Agreement of 1978. It has also addressed the impact of a lower budget level and identified some of the desirable programs which could not be supported even at the higher of the above levels.

The organization of this Report is as follows. Section II contains a Summary of the Subpanel's planning recommendations. Section III has a discussion of the Scientific Issues. Section IV provides a summary of facilities in High Energy Physics, including those in operation, those under construction and those proposed. Section V provides Technical Evaluation of facility construction and development projects at the U.S. accelerator laboratories. Section VI discusses that part of the elementary particle physics program not based on accelerators (Non-Accelerator Physics). Section VII discusses in detail the proposed high energy programs at the various budget levels (Facility Recommendations and Long Range Plans). Section VIII deals with the area of international collaborations and Section IX discusses ways of improving the long-range planning process itself.

SECTION II

SUMMARY

It is very likely that history will show the decade of the 1970's to have been one of the greatest periods of progress in our understanding of the basic constituents of matter and the forces acting between them. This progress was made possible by a variety of crucial experimental discoveries and theoretical contributions in which U.S. high energy physicists played the major role. The future of high energy physics promises to be as exciting and productive as its profoundly successful recent past.

The U.S. High Energy Program remains strong, but it faces vigorous competition from other regions of the world. To maintain its vitality and preeminence over the next decade it requires the following major ingredients: (1) strong exploitation of existing facilities; (2) the expeditious completion of construction projects which will expand these facilities over the next few years; (3) the construction of a substantial new facility to be ready for research by the end of the 1980's; and (4) the vigorous pursuit of a wide range of advanced accelerator R&D programs in preparation for the design and construction of a higher energy accelerator which would probably be initiated near the end of this decade.

The Subpanel has considered how best to accomplish these goals under two different budgetary assumptions; namely, average yearly support levels of $440M DOE, $35M NSF, and $395M DOE, $34M NSF (FY 1982 dollars). It has also considered the impact of a yet lower support level of $360M DOE and $32M NSF. Its recommendations for the highest of these levels (assumed to be reached from the present lower levels by FY 1984 or at the latest FY 1985) are as follows (no priority order is implied):

--Construction of ISABELLE at Brookhaven with completion by the end of this decade.

--Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

--Expeditious completion of the Energy Saver at Fermilab.

--Implementation of the Tevatron II fixed-target program at Fermilab in all three experimental areas.

--Completion of the Tevatron I proton-antiproton collider at Fermilab, including the provision of a major detector facility.

--Continuation of the major R&D effort at SLAC on the Stanford Linear Collider, with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in

FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for physics research.

--Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating structures, superconducting RF cavities and novel means of acceleration.

This program, although lean in many respects, enables the U.S. to maintain an active and healthy position in the field through the end of this decade and into the 1990's.

For the lower budget level of $395M DOE and $34M NSF, the program differs in that the present ISABELLE project cannot continue*, but is replaced by the following recommended element:

--With the present ISABELLE project terminated, a start by the mid-1980's on a new high energy construction project of more modest financial scope. Although it is premature to recommend a specific direction at present, the Subpanel is aware of a number of interesting concepts currently being considered by the community and urges intensified discussion, development, and R&D aimed at selection and authorization by the middle of the decade.

Such a facility would have to be lower in cost than the ISABELLE project. While this $395M program would be substantial, its scope would be significantly reduced from that envisaged at the higher budget level.

A sustained $360M DOE/$32M NSF level would require termination of ISABELLE, make the initiation of a new project in the mid-1980's impossible, and lead to one or more other drastic reductions in the previously recommended programs. Many physicists would undoubtedly leave the field and this irretrievable loss of competent and highly-trained personnel would perhaps be the most damaging result. This would deal a staggering blow to the health of the U.S. High Energy Physics Program which would necessarily be headed for a serious decline.

Besides developing the above long range strategies, the Subpanel has also considered other planning issues, including non-accelerator elementary particle programs, international collaboration, and long-range planning mechanisms, all of which are discussed in the body of the Report.

---

* Between a steady $(395, 34)M level and the higher level, which assumes an early approach to $(440, 35)M, there is a difference of opinion within the Subpanel as to where the line should be drawn for abandonment of the ISABELLE project. Indeed some members felt that inclusion of ISABELLE construction even at the higher $(440, 35)M level would jeopardize funding for the other elements of the national program. Others felt that the Project must be sustained even at the $(395, 34)M funding level. The majority was of the opinion that it should be included at the higher level and not at the lower.

SECTION III

SCIENTIFIC ISSUES

A. Status of the Field and General Approach

In the 1970's there was extraordinary progress in our understanding of the constituents of matter and their mutual interaction. This progress has occurred in at least four interconnected areas. First, experiments have established the existence of several new fundamental fermions: the charm quark, the bottom quark, and the tau lepton. Second, we now have the SU(2) X U(1) theory of the unification of the weak and electromagnetic interactions; this theory approaches quantum electrodynamics (QED) in its predictive power. Third, the concept of the quark-parton structure of hadronic matter has been extended enormously both by experiment and by phenomenological theory. Fourth, we now have a candidate fundamental theory for the strong interaction, quantum chromodynamics (QCD), which rivals QED in its beauty, economy of formulation, and potential range of validity. Together these have led to what is often called the "standard model" for elementary particle physics.*

Although some aspects of this theory need crucial experimental tests, it does provide a comprehensive reference to which alternative theories and models can be--and inevitably are--compared. The standard model also provides a way to classify the potential significance of experiments and of future facilities.

However, we must be ready for surprises. It is often said--and history bears witness to its correctness--that the reasons stated in advance to justify construction of a facility turn out not to be the most important ones. One may argue that the present situation with respect to QCD and SU(2) X U(1) is similar to the status of QED in the past; that is, they are becoming well-verified theories. Hence, one may argue that the area where surprises can occur is much smaller than it was in the past. Nevertheless, even within the standard picture, there is an additional force not clearly associated with gauge interactions. This is the so-called Higgs sector, a somewhat awkward feature of the theory. There can be every expectation of surprises occurring in that general domain of phenomena.

We, therefore, identify three distinct ways of viewing the future contributions of the new facilities. We may view them in the context of: (1) testing the standard model; (2) searching for and/or discovering phenomena which at present are reasonably well-defined but which either contradict or lie beyond the standard model; and (3) discovering genuine surprises, which by definition cannot be discussed in specific terms at present. The relative weight to be given these three categories is a subjective question. Based on the above discussion, we caution that the relative weight may not be the same as one would have given even 5 years ago. Beyond this cautionary statement, the question of how to choose the balance is one of scientific instinct.

---

*We define the standard model to mean three, and only three, generations of quarks and leptons, SU(3) X SU(2) X U(1) gauge theories for strong, weak, and electromagnetic forces, one Higgs boson, and CP violation via complex parameters in the mass matrix. Neutrino mass and mixing, as well as proton decay, are left optional.

In the following Subsection, we discuss the ways future facilities might contribute to the field. We shall see that from the physics point of view, an optimal high energy program requires a diversity of approach, with fixed-target physics, electron-positron colliders, electron-proton colliders, proton-proton colliders, proton-antiproton colliders, and major non-accelerator experiments all serving as important contributors.

B. Contributions of New Facilities Within the Context of the Standard Model

There are two crucial tests facing the thus far successful SU(2) X U(1) gauge theory of weak and electromagnetic interactions. The first is the discovery of the carriers of the weak force, the intermediate bosons $W^{\pm}$ and Z^0, and the second is the precision measurement of $\sin^2\theta_W$, the main free parameter of the model. While one anticipates the Z^0 to be discovered at CERN's 270 x 270 GeV protron-antiproton collider in the near future, significantly higher luminosity and/or energy may be needed to identify the $W^{\pm}$. Incisive study of the Z^0 (as well as W) production dynamics also requires these higher luminosities and energies. This study is not only a probe of the weak force, but is also a sensitive probe of the strong interaction at short distances. There are many other properties of the particles such as mass, width, and decay modes into various final states, which are of vital theoretical interest. In the case of the Z^0, these detailed properties are most easily studied with electron-positron colliders (such as LEP at CERN, SLC at SLAC or CESR II at Cornell). Precision measurements of the Z^0 width yield a count of the number of neutrinos in nature. Measurements of partial widths, angular distributions, and polarization asymmetries sensitively test the structure of the weak coupling. Electron-proton colliders will also make powerful contributions, especially with regard to the mass and detailed couplings of $W^{\pm}$ to quarks and leptons, which are competitive with information obtainable from a hadron-hadron collider.

Study of W pair production is very important because it tests in a basic way the gauge aspect of the SU(2) X U(1) theory. With the full energy of LEP, this process can be studied; indeed this has been a principal motivation for its high design energy. This process may also be accessible for study at ISABELLE and TeV I, although in either facility the experiment is difficult.

The second important goal is the measurement of the basic parameter, sin$^2\theta_W$, of the electroweak theory. Precise information on sin$^2\theta_W$ is also relevant to grand unified models of electroweak and strong interactions. This measurement must be done at high and low energy scales in order to measure radiative corrections. At high energies, polarization asymmetries in Z^0 decay, as measured in electron-positron facilities such as SLC, are a sensitive probe. At low energies, a variety of facilities are of importance, especially lepton beams such as will exist at TeV II, or even polarized electron-electron scattering at the SLAC linac.

Parameters of the mass and mixing matrix which determine the pattern of quark weak decays also need measurement. Here again fixed target facilities such as TeV II and AGS, where subtle issues may be addressed, are essential. In the case of the heavy quarks, SPEAR remains an excellent place to study couplings of the c-quark, while CESR and DORIS are the facilities at present for the study of b-decays. Many heavy quarks are produced in the TeV II secondary beams, and in the long run these may become a very fruitful source of information as well. Semileptonic production of b-quarks--especially at an electron-proton collider--may be yet another source of information on weak couplings of the b-quark.

Investigation of the weak couplings of the t-quark depends on its mass. Electron-positron colliders are optimal, if there is adequate energy to produce t pairs. For masses up to 100 GeV, the proton-proton and proton-antiproton colliders may provide information. Currently proposed electron-proton colliders are effective if the t-quark mass is less than 50 GeV.

In the standard model, CP violation has a specified origin in the quark mixing matrix. Again, a variety of facilities may contribute, notably TeV II, AGS, and CESR.

Neutrinos may have masses and mixing parameters themselves. This is an open question within the standard model but arises naturally in some extensions. An enormous range of phenomena must be studied with great sensitivity to settle these issues. Large underground or underwater detectors, nuclear reactors, and meson factories offer a broad range of possible neutrino production and interference effects. Fixed-target accelerator programs facilitate experiments that probe other possible ranges of masses and different mixings.

There is also, within the standard model, the task of finding the three most prominent missing particles--the tau neutrino, the t-quark and the Higgs boson. Direct detection of the tau neutrino appears to require TeV II. With respect to the t-quark and Higgs boson, no one knows what masses these particles have. They could be anywhere from 10-30 GeV at the low energy end to hundreds of GeV at the upper energy end. For low masses (less than about 40 GeV) electron-positron colliders appear optimal for both Higgs boson and top quark. For higher masses (up to 100 GeV) the proton-proton and proton-antiproton colliders may be excellent tools for finding the top quark. For the highest Higgs boson masses, LEP or an even higher energy electron-positron collider may be needed.

Study of the strong interactions focuses on refining the tests of QCD, the present leading candidate for a fundamental theory. The qualitative confirmation observed most cleanly in electron-positron and lepton-nucleon collisions (and also supported by lepton-pair and high transverse momentum hadron production in hadron-hadron collisions) will be subjected to stringent tests when the energy ranges are dramatically extended by the future colliders. To the classical tests of electron-positron total cross section, gluon jet production, study of quarkonium properties, and scaling violations in lepton-nucleon collisions (available in the future to electron-proton colliders), one may add gluon jet production in electron-proton collisions, and possibly, in proton-proton and proton-antiproton collisions. Production of dileptons--especially those from Z^0 or $W^\pm$ decay--and of jets and direct photons of extremely high transverse momentum are excellent tools for probing QCD at the smallest distance scales. The secondary beams of TeV II will allow a detailed study of the internal quark structure of a great many hadrons, as revealed by lepton pair production. Further tests of QCD and hadron structure will certainly be devised as the theory is improved.

The properties of the strong force at larger distances--including the question of quark confinement--are a vital element as well. Quark searches should be carried out as each new energy frontier is opened. Also the sensitivity of non-accelerator quark searches in bulk matter should be improved. The nature of the strong force at long distances is certainly reflected in hadron spectroscopy, and the existence of predicted structures such as gluonium is best studied at lower energy accelerators such as SPEAR, AGS, or the SLAC linac. Meson spectroscopy at SLAC and AGS energies is far from complete, and such work should be important in comparing properties of light quarks to heavy quarks.

High energy soft collisions at TeV II and all colliders probe a unique combination of long and short distance physics. This involves the measurement of elastic, total, and various diffractive cross sections, multiplicity and correlation studies, and inclusive spectra measurements. This subject should be codified into a comprehensive phenomenology; an example is the Reggeon calculus. The phenomenology should eventually be comprehended in terms of a fundamental theory of the strong interaction such as QCD. While such studies are in a nascent state, one can expect growing interest in the subject over the coming years.

Another area of interest lies in the possible production of quark-gluon plasma in high-energy heavy-ion collisions. This subject is new and at present being actively pursued. The capability of ISABELLE eventually to store heavy ions makes this an attractive prospect for the longer-range future.

C. Contributions of the Facilities to Physics Beyond the Standard Model

Any discussion which goes beyond the standard model must be regarded in a more tentative way than the discussion in the previous Subsection. The topical theoretical concepts (extended gauge groups, technicolor, supersymmetry, compositeness) which imply excursions from--or modifications of--the standard model lack coherence and internal consistency at present. They are subject to relatively frequent revisions. A recurrent theme of these efforts is dissatisfaction with the conventional description of the forces associated with the Higgs sector. As we mentioned before, it is especially in this area that one can most confidently expect surprises. While the theoretical ideas are unreliable, they do provide prototypes for what might actually be present, are guideposts for experimental searches, and may be reasonable bases for comparing the possible contributions of future facilities.

The electroweak SU(2) X U(1) theory may not be correct. There may be no intermediate boson or there may be extra bosons, especially Z^0's. Proton-proton or proton-antiproton colliders such as ISABELLE and TeV I are excellent facilities for carrying out a search for such heavier Z^0's. The range of mass sensitivity runs, in the context of gauge theories, from present lower limits of 30-60 GeV to somewhere near 300 GeV for both TeV I and ISABELLE, with the high luminosity of ISABELLE a significant advantage. In the context of the original Fermi theory, the sensitivity extends to 900 GeV for the TeV I collider. Electron-proton collisions also provide a quite sensitive probe of heavier W's and Z's. Direct production of heavier Z^0's in electron-positron machines is limited to the total maximum energy, but the existence of such particles at up to nearly twice that mass could be inferred from interference effects.

Alternatives to the standard picture typically predict a proliferation of new particles. The list includes heavier quarks, heavier leptons, charged Higgs bosons, leptoquarks, technihadrons, axions, and supersymmetric partners of conventional particles. Most (but not all) of these can in principle be found in electron-positron colliders such as SLC or CESR II, provided the mass of such particles is less than 40 or 50 GeV (the LEP II project may eventually extend this number to 100 GeV). Some others, such as leptoquarks, may be better found in electron-proton colliders. Conjectured particles with masses in the 50-200 GeV range may be produced with proton-proton or proton-antiproton colliders. Production models for many of these particles--including some of mass 200-400 GeV--indicate that the cross-sections are high enough for them to be detected. Massive axial gluons, associated with spontaneous breakdown of a chiral SU(3) X SU(3) color group, might be detectable up to a mass scale of 400-600 GeV. Detection of any of these particles in the midst of a large background is difficult.

Some approaches to improving and extending the standard model make definite predictions of new phenomena at distances so short or energies so high that relevant accelerator experiments are inconceivable on any reasonable time or budget scale. However, these phenomena can be explored by other means.

The simplest grand unified models of electroweak and strong interactions predict the existence of proton decay and magnetic monopoles. These are each the subject of major non-accelerator experiments. (See Section VI).

It is possible that particles we today consider pointlike may be composite extended structures. This is at present a matter of serious theoretical consideration, extending to many of the particles we have discussed. The search for compositeness is a basic driving force toward higher energies. Most of the new facilities can be expected to set strong limits on the scale of compositeness or to directly observe the effects. The sensitivity level for most of these facilities is in the several hundred GeV range.

D. Intangibles

The kind of analysis made in the preceding Subsections cannot be blindly trusted. History teaches that the course of scientific progress is tortuous, with discoveries of great significance coming from unexpected directions. For example, according to the classifications we have given, the best machine for discovery of the upsilon would have been an electron-positron collider; nevertheless, it was found in proton-nucleon collisions. One could hardly have anticipated at the time of construction of the AGS that discovery of CP violation would be one of its greatest contributions.

While by definition, we cannot talk in detail about surprises, the general capabilities of a given facility for providing surprises may be considered--and these are of great importance. Each facility has its own, often unique, capabilities. An evident capability of fixed-target facilities is their high effective luminosity and concomitant diversity of approach. One measure of this is that, even from the point of view of exploratory "frontier" science (as opposed to programmatic studies), such facilities approach obsolescence only very slowly. The discovery of the J/psi at the AGS occurred 14 years after commissioning of the machine. At this time highly exploratory experiments are being performed at the oldest facilities, namely the SLAC linac and the AGS. One has no reason to expect less longevity of the TeV II program at Fermilab.

With regard to colliders, the oft-stated and well proven argument on behalf of electron-positron colliders is "anything charged (with mass less than the beam energy) will be produced." With the next generation of electron-positron colliders, this can be generalized to "anything, charged or neutral, with electroweak interaction, will be produced." Furthermore, backgrounds tend to be relatively small, so that detection problems are minimized.

With the advent of QCD, a similar argument applies to the high energy proton-proton and proton-antiproton colliders: "anything colored will be produced." There the background is higher but the center-of-mass energy is in practice an order of magnitude greater. Thus only the hadron colliders such as TeV I and ISABELLE will have access to the highest energy processes.

Electron-proton colliders combine the clean signal of a structureless electron probe with tne high energy and rate associated with a proton projectile. Here, anything that mixes leptons and quarks or lets them interact will be detected. In all cases, it is self-evident that the exploratory capabilities of the colliders are very strong.

Sophisticated non-accelerator facilities for particle physics also offer opportunities for discoveries in unexpected directions. They can use cosmic rays as a source of particles at energies far above those accessible to accelerators. The small flux of these highest energy particles must be compensated by use of enormous detectors and new detection techniques. Such large systems can also look for processes far rarer than those detectable by normal techniques.

SECTION IV

SUMMARY OF FACILITIES IN HIGH ENERGY PHYSICS

A. The U.S. Program

The U.S. High Energy Physics Program largely involves the exploitation of particle accelerators and storage rings located at three national laboratories supported by the U.S. Department of Energy (Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and Stanford Linear Accelerator Center) and the Cornell University Newman Laboratory of Nuclear Studies supported by the National Science Foundation. A much smaller but important component of the program involving elementary particle experiments which do not use accelerators or storage rings is described in Section VI. This Section summarizes the properties and physics activities associated with the various U.S. accelerator facilities operating, under construction, and proposed. In addition accelerator facilities in other countries are also briefly described.

1. Fixed-Target Program at BNL

The fixed-target experimental program at BNL is based on the 30 GeV proton synchrotron, the AGS, which provides up to 10^{13} protons per pulse every 1.2 seconds for neutrino physics and, with a one second flattop, every 2.5 seconds for electronic counter experiments. A development effort is under way to accelerate polarized protons, which would provide a unique capability.

The experimental program principally focuses on studies of weak interactions via neutrino reactions and K meson decays, hadronic spectroscopy with the multiparticle spectrometer including searches for gluon bound states, and hyper-nuclear physics.

The AGS is to become the injector for the large proton-proton storage ring ISABELLE presently under construction.

2. Fixed-Target Program at SLAC

The SLAC linac is the highest energy and highest intensity electron accelerator in the world. Its energy is being upgraded to about 50 GeV in connection with the Stanford Linear Collider (SLC) development. Its future experimental program focuses on further electron-proton and electron-nuclear scattering experiments with and without polarized electrons, beam dump experiments, studies of charmed particle properties and photoproduction with the hybrid bubble chamber facility, and studies of the spectroscopy of K* mesonic systems with the LASS spectrometer.

3. Fixed-Target Program at Fermilab (Tevatron II Project)

The physics program at Fermilab has up to the present time used beams produced by a 400 GeV synchrotron. The Energy Saver Project involves the construction of a ring of superconducting magnets in a position just below the present synchrotron to provide cost effective operation up to an energy of 500 GeV. The Energy Saver is scheduled to begin operation in late 1982 or early 1983. The superconducting magnet ring has been designed to reach energies up to 1000 GeV.

The Tevatron II project is a program to upgrade the beam lines and experimental halls to 1000 GeV operation. The time scale is such as to make some beam lines available in late 1983 and complete the upgrade by about the end of 1985. The cycle time of the new accelerator (1 minute) will be five times as long as that of the present machine and the intensity per cycle will be about the same ($>2 \times 10^{13}$ particle/pulse). However, the increased energy will provide time-averaged particle fluxes as large or larger than those in the energy region presently available with 400 GeV protons. Furthermore, useful particle fluxes will be available up to about 800 GeV, and the 20 second flattop will improve the duty cycle by about a factor of four.

These new facilities will permit a rich and diversified experimental program. Some of the ingredients of that program will undoubtedly be studies of neutrino interactions including detection of the ν_τ and searches for oscillations, studies of structure functions of hadrons, heavy quark spectroscopy, and tests of QCD through studies of jet phenomena.

4. SPEAR

The SPEAR electron-positron storage ring at SLAC operates with a center-of-mass energy between 2.5 and 8 GeV. Since the turn-on of the PEP storage ring, its operating time has been divided equally between high energy physics and synchrotron radiation research. The only detector presently projected for operation at SPEAR after the spring of 1982 will be the Mark III magnetic detector recently installed in the West Interaction Area. Systematic studies of states involving the c-quark, and states produced by radiative decay of the J/psi will provide a rich program not duplicated anywhere else in the world.

5. CESR

The Cornell electron-positron storage ring CESR has been operating since 1979, principally in the center-of-mass energy range 9 to 12 GeV, although higher energies are possible. Recent modification of the interaction region has produced a significant luminosity improvement. There are two interaction regions of which one is occupied by a large general purpose magnetic detector and the other by a non-magnetic detector which emphasizes high resolution electromagnetic calorimetry.

The CESR energy range is ideal for the study of the physics of the b-quark, and the major thrust of both experiments will continue to be the spectroscopy of the upsilon $b\bar{b}$ states and the mesons and baryons containing one b-quark. This program will undoubtedly require a number of years for its completion.

6. PEP

The PEP electron-positron storage ring which can provide center-of-mass energies between 8 and 30 GeV went into regular physics operation at SLAC in early 1981. During the summer of 1981, the interaction regions were modified to a mini-beta configuration and substantial luminosity improvements are expected. Five large detectors are either installed or in the process of installation in the five interaction regions available for physics research. These detectors embody a very considerable diversity in capability, and should produce a correspondingly diverse research program. The principal effort at PEP will involve precise and careful tests of both electroweak and QCD predictions, the detailed study of quark and gluon fragmentation, systematic studies of two-photon processes, and possibly c- and b-quark spectroscopy.

7. Tevatron I Project

The Tevatron I project (TeV I) at Fermilab is a proton-antiproton collider which will provide a luminosity of about 10^{30} cm^{-2} sec^{-1} at a center-of-mass energy of 2000 GeV. A more detailed technical description and assessment are given in Section V.

The start-up date for this facility is expected to be in 1985. A large and sophisticated general purpose detector is under construction for one of the available interaction regions and a modest experiment is contemplated for the other available interaction area. TeV I will have to share its operating time with the fixed target program (TeV II). It should provide a window into very high energy processes including production of $W^{\pm}$ and Z^0 intermediate bosons, studies of jets at high energy, and searches for surprises and new phenomena.

8. ISABELLE Project

ISABELLE is a proton-proton collider at Brookhaven designed for a maximum center-of-mass energy of 800 GeV and a luminosity of about 10^{33} cm^{-2} sec^{-1}. A technical description and assessment are given in Section V. Start up of ISABELLE, if one assumes a reasonable funding rate, would come around 1988. ISABELLE will have six interaction regions and will be a dedicated facility not sharing time with other programs. The physics areas addressed by ISABELLE include studies of W and Z production, dilepton and high transverse momentum jet production, searches for the t-quark and for a variety of other particles conjectured by current theoretical ideas, and generally searches for new phenomena.

The high luminosity and flexibility provided by many interaction regions should provide the opportunity for experiments of high sensitivity to very rare phenomena, provided that backgrounds can be successfully controlled.

9. SLAC Linear Collider (SLC) Project

The SLC project is a program at SLAC to achieve collisions of high-intensity micron-sized bunches of electrons and positrons with a center-of-mass energy of about 100 GeV. These bunch pairs arrive at the collision point at the pulse rate of the SLAC linear accelerator (180 per second) and after the collision the electrons and positrons are disposed of in a dump. The SLC project is a proposed construction project whose justification is twofold: (1) development of techniques for construction of linear electron-positron colliders whose energies might reach beyond those possible with circular colliders; and (2) an early look at the physics of the Z^0. A more detailed description and technical assessment are given in Section V.

B. The European Program

The European program in experimental High Energy Physics is centered at CERN in Geneva, Switzerland, and DESY in Hamburg, West Germany. CERN has 3,600 staff members, a yearly budget of approximately 600 MSF, and roughly 1,800 high energy physicists are involved in the program. The corresponding numbers for DESY are a staff of 1,100 and a yearly budget of approximately 130 MDM. Although DESY is a national laboratory, it is exploited internationally with groups from 11 nations and a total of 400 physicists participating in the program. A substantial European effort in non-accelerator based research is discussed in Section VI. At CERN, the center of activity is the SPS which provides 450 GeV protons for the fixed-target program and also serves as a 540 GeV proton-antiproton collider. A vigorous program is also carried out at the ISR utilizing both proton-proton and proton-antiproton collisions. CERN plans to phase out the ISR with 1983 as the last year of operation in order to free funds and personnel for the LEP project. Furthermore, CERN has decided to construct a low energy proton-antiproton facility, LEAR, based on technology developed for the high energy proton-antiproton project. For its future program CERN will construct a large electron-positron colliding ring, LEP. The first stage of this project aimed at providing electron-positron collisions at 100 GeV in the center-of-mass in four interaction regions has been approved by the CERN council. According to the CERN schedule, the first beams are expected toward the end of 1987 or the beginning of 1988.

The program at DESY is based on the electron-positron colliding rings DORIS and PETRA with the 7 GeV synchrotron used as an injector and to provide test beams. A proposal for HERA, a large electron-proton colliding beam facility with a center-of-mass energy of 314 GeV, has been prepared and will be submitted formally to the German Government shortly.

The following discusses the facilities mentioned above in more detail.

1. CERN

a. Fixed-target operation of the SPS

The SPS is the mainstay of the experimental program at CERN. The maximum energy has been upgraded to 450 GeV. The intensity with two batches injected from the PS is now about 2 x 10^{13} protons approximately every 10 seconds. The SPS has a very strong experimental program. The West Experimental Area will be upgraded to provide two high energy, high quality secondary beams in addition to several test beams.

New neutrino beams for beam dump experiments and for neutrino oscillation experiments are being constructed. The fixed-target program is expected to remain strong during LEP construction.

b. The proton-antiproton colliding project

The aim of this project is to produce proton-antiproton collisions at a center-of-mass energy of 540 GeV with a peak luminosity of 10^{30} cm^{-2} sec^{-1} in two intersection regions. This is achieved as follows: 26 GeV/c protons from the PS are used to produce 3.5 GeV/c antiprotons which are stored and stochastically cooled in an accumulator ring. After a sufficient number have been accumulated, the cooled antiprotons are reinjected into the PS, accelerated to 26 GeV, and transferred to the SPS.

The construction has been completed and the facility is now being commissioned with two of the five approved experiments taking data. High intensity bunches have been injected and stored with long lifetimes and tuneshifts of the order of 0.0025 have been observed. A luminosity of greater than 10^{27} cm^{-2} sec^{-1} has been obtained, and a luminosity of 10^{28} cm^{-2} sec^{-1} should be reached in the near future. The intention is to run proton-antiproton roughly 1/3 of the available time, but the precise time division between fixed target operation and proton-antiproton collisions will depend on the physics output.

c. The ISR

At present the ISR has a strong experimental program. The machine performs well with peak luminosities exceeding 10^{32} cm^{-2} sec^{-1} for proton-proton collisions in the low beta intersection region at a center-of-mass energy of 62 GeV. This has been achieved with the use of superconducting quadrupoles to focus the beam. A luminosity of 9 x 10^{26} cm^{-2} sec^{-1} has been reached in proton-antiproton collisions, and substantial amounts of data have been collected. The present intention is to turn off the ISR at the end of 1983.

d. Medium energy physics

The medium energy physics program research is fed by the PS. The emphasis is on antiproton and kaon physics at very low energies. As a spinoff of the proton-antiproton project, CERN is constructing a low energy antiproton facility, LEAR. Cooled antiprotons from the accumulating ring will be reinjected in batches into the PS, decelerated to 0.6 GeV/c and transferred to a small storage ring, LEAR. LEAR has different modes of operation. It can be run as a stretcher ring with 1000 s spill time delivering an external beam of 10^6 antiprotons per second with a momentum between 2.0 GeV/c and 0.1 GeV/c. It can also be run as a storage ring, either as a minicollider (proton-antiproton at 4.4 GeV) or as a source of antineutrons via the charge exchange reaction $p\bar{p} \rightarrow n\bar{n}$. Compared to present external beams, LEAR represents a large improvement in beam purity, intensity, and duty cycle. LEAR is expected to start operations in 1983.

e. LEP

LEP is an electron-positron storage ring with a circumference of 26 km and at most eight interaction regions. Phase 1 of LEP, aimed at providing 50 x 50 GeV electron-positron collisions in four intersection regions, was approved by the CERN council at its October meeting. The predicted peak luminosity is 10^{31} cm^{-2} sec^{-1}. The CERN schedule calls for the first beams by the end of 1987.

In Phase 1, only 1/6 of the available space along the ring will be equipped with conventional cavities. With the full complement of conventional cavities, LEP will be able to reach 2 x 85 GeV in the center-of-mass. However, if the superconducting technology is developed by the end of the 1980's, an energy upgrading of LEP will presumably be based on this technology. The center-of-mass energy could be 250 GeV with a gradient of 5 MV m^{-1}. A four-cell superconducting cavity has reached 2.5 MV m^{-1}.

It is premature to speculate on what the next step at CERN might be. However, it seems rather natural to install in the LEP tunnel a superconducting magnet ring suitable for storing 5 to 10 TeV protons. This would provide a rather impressive energy range for electron-proton or proton-antiproton collisions.

2. DESY

a. DORIS

The electron-positron collider ring DORIS is now being rebuilt with more RF added to raise the maximum center-of-mass energies to 12.0 GeV.

Both interaction regions will be equipped with mini-beta optics with an expected integrated luminosity of 300-400 nb^{-1} a day. DORIS will resume operation in May 1982 with two complementary detectors, a large solenoid detector ARGUS, and the Crystal Ball detector from SPEAR. A detailed study of bound $b\bar{b}$ states and of b-decays will be carried out.

b. PETRA

The electron-positron colliding ring PETRA has so far operated at center-of-mass energies between 10 GeV and 36.7 GeV. The installation of a mini-beta system in all four interaction regions has resulted in a peak luminosity well above 10^{31} cm^{-2} sec^{-1} and an average integrated luminosity between 400 and 600 nb^{-1} per day per interaction region.

A program has been initiated aimed at increasing the maximum center-of-mass energy to about 45 GeV in two steps. An increase from 36.7 GeV to 40.5 GeV will be achieved by mid-1982 with a doubling of the RF power from 4MW to 8MW. With the installation of RF cavities in the two remaining long straight sections the energy will be increased from 40.5 to about 45 GeV by early 1983.

Superconducting RF cavities are required to increase the energy beyond 45 GeV up to a maximum of 60-70 GeV. A single cell superconducting cavity built by Karlsruhe has successfully been tested with high RF power and will be installed at PETRA in 1982. DESY in collaboration with CERN and Wuppertal has a program to build and test multicell cavities.

c. HERA

DESY has proposed the construction of a large electron-proton colliding beam facility, HERA, designed to collide either electrons or positrons of 30 GeV nominal energy with protons of energies up to 820 GeV yielding 314 GeV in the center-of-mass and a maximum momentum-transfer-squared, Q^2 of 98,000 GeV^2. This is equivalent to an electron beam of 52 TeV on a stationary target. The design luminosity of around 10^{32} cm^{-2} sec^{-1} is sufficient to explore both charged and neutral current processes in the Q^2 region up to 40,000 GeV^2.

This machine has received strong support from the European High Energy Physics community and has been recommended for construction by a Government committee set up to evaluate the merits of HERA and other large scientific projects. This committee stressed the need for international collaboration both in the construction and the exploitation of HERA. The Senate of the City of Hamburg, which will contribute 10 percent to the total construction cost, has approved the project.

A detailed technical proposal for HERA has been prepared in collaboration with some 22 European institutions and will be formally submitted to the German Government shortly. The construction time of the project is estimated to be 7 years.

C. The Japanese Program

Elementary particle research in Japan has so far been carried out at the 12 GeV proton synchrotron at KEK. The future program involves the construction at KEK of a large storage ring complex, TRISTAN, designed to collide electrons and positrons and, at a later stage, electrons (or positrons) with protons. The TRISTAN ring will have a total circumference of 3016m and four interaction regions. An electron-positron colliding ring capable of exploring center-of-mass energies up to 60 GeV will be built. A maximum luminosity of 1.5×10^{31} cm^{-2} sec^{-1} is expected at 57 GeV. This project has been authorized and construction is now underway with the start of the experimental program scheduled for April 1986.

In a second stage a proton ring with 4.5 tesla superconducting magnets will be added. This will provide a proton energy of 300 GeV. Collisions of the proton beam with the 25 GeV electrons will yield a center-of-mass energy of 173 GeV, equivalent to 16 TeV for a fixed-target electron machine. If authorized by 1984, this project should be completed around 1990.

KEK, in collaboration with industry, has a strong program to develop superconducting cavities. High field superconducting magnets are developed in collaboration with BNL, Fermilab, and industry.

D. The USSR Program in High Energy Physics

Historically, research in high energy physics in the USSR has been carried out at five facilities: The Institute for High Energy Physics (IHEP), Serpukhov; the Joint Institute for Nuclear Research (JINR), Dubna; the Institute of Nuclear Physics (INP), Novosibirsk; the Institute for Theoretical and Experimental Physics (ITEP), Moscow; and the Institute of Physics (IP), Yerevan. The accelerator facilities at three of these (JINR, ITEP, and IP) are obsolete by modern standards, with center-of-mass energies of the available particle beams below 5 GeV, and are no longer considered major facilities; although Dubna does maintain an active program in high energy physics. The two major facilities now active for research in high energy physics in the Soviet Union are IHEP and INP.

1. IHEP (Serpukhov)

The major proton facility in the USSR is the 76 GeV synchrotron at IHEP. From 1967 until 1972, before Fermilab began operating, this accelerator was the highest energy proton source in the world. Although the proton beam intensity is low by Fermilab and CERN SPS standards, an active program of physics is being pursued. The research program is similar to the one at the AGS at BNL, with emphasis on large, relatively permanent experimental set-ups. There is a neutral beam in which some unique work on charm production by hadrons is being done and a neutrino counter experiment which is in limbo until the new booster comes into operation to increase the intensity. One group has been working on K^0 regeneration experiments for 10 years, and a large multiparticle spectrometer set up has been looking at charmed particles. There is a group doing emulsion work which has provided the cold emulsions for the neutrino search for charm in the 15-foot Fermilab bubble chamber. SKAT, the large propane chamber for neutrino physics, is still in operation as is MIRABELLE for hadron physics. The bubble chamber scanning and measuring facilities at both Dubna and Serpukhov are extensive and recently upgraded.

In the future the 76 GeV Synchrotron will become the injector for a multi-TeV accelerator/storage ring, UNK. Phase I of UNK, which was authorized for construction in 1981, includes a 400 GeV booster synchrotron using conventional magnet technology and a 400 to 3000 GeV (3 TeV) synchrotron using superconducting magnets whose design is similar to Fermilab's. Phase I will provide fixed target beams to 3000 GeV maximum incident energy (75 GeV center-of-mass) and proton-proton colliding beams of 400 x 3000 GeV (2190 GeV center-of-mass energy). Phase II, which is still in the planning stage, will provide 3000 x 3000 GeV proton-antiproton colliding beams and, with the addition of a second superconducting magnet ring, 3000 x 3000 GeV proton-proton collisions. UNK, if successfully completed, will provide a high energy physics research capability for the USSR in the 1990's far beyond any facility being planned elsewhere.

2. JINR (Dubna)

Although the accelerator facility at Dubna is not competitive by modern standards, the high energy physics program at this laboratory remains active. The group is allocated approximately 30 percent of the research time at Serpukhov and maintains a group of physicists on site to support this research. At Dubna, there is work on quark models of the nucleus using heavy ion beams. There is also some fine polarized jet target research which employs a very advanced and effective solid state detector array.

3. INP (Novosibirsk)

The Institute of Nuclear Physics (INP) at Novosibirsk is the center of lepton interaction physics in the Soviet Union. The research centers around the VEPP-4 electron-positron storage ring, which is designed for a maximum energy of 7 x 7 GeV (14 GeV, center-of-mass energy) and has reached 3.1 x 3.1 GeV so far. This device has been used to make precision measurements of particle masses in the J/psi range (4 GeV). An older storage ring, VEPP-3, is used as an accumulator/injector for VEPP-4 which allows the electrons and positrons to polarize before injection. The main detector facility employs a 400-ton magnet with the field perpendicular to the plane of the particle orbits. While the physics research program at INP is considered modest by western standards, the contributions of INP to the development of accelerator technology have been exceptional. INP is reported to be presently involved with design of an antiproton source for use with UNK. This effort stems from the pioneering work at INP on electron cooling of proton beams.

SECTION V

TECHNICAL EVALUATION

The Technical Subgroup of the Subpanel, P. Reardon, E. Knapp, W. Schnell, A. Sessler with consultants R. Lundy, J. Rees, and C. Taylor visited Brookhaven on October 8-9, 1981. A Subgroup of P. Reardon and A. Sessler with consultants R. Jameison and J. Rees, visited Fermilab on October 1-2, 1981. W. Schnell of the Technical Subgroup visited Cornell on October 12-13, 1981, and two more members of the Technical Subgroup, E. Knapp and P. Reardon with consultants A. van Steenbergen, E. Paterson, and P. Wilson visited Cornell on November 19-20, 1981. E. Knapp, P. Reardon, and A. Sessler of the Technical Subgroup with consultants P. Sargent and A. van Steenbergen visited SLAC on November 3-4, 1981, and W. Schnell of the Technical Subgroup visited SLAC on November 30 - December 1, 1981. Comments on these projects are presented in this Section.

A. Description of the ISABELLE Project

ISABELLE is a proton-proton collider designed for a maximum energy of 400 GeV x 400 GeV. Proton beams circulate in opposite directions in two separate rings each of 3,834 meters circumference. Superconducting dipole magnets with 5 tesla field are required to achieve this. Separate rings, crossing each other only where collisions are desired, permit the use of unbunched direct current beams. This eliminates a number of intensity-limiting effects and leads to a higher beam-beam limit than would be obtained with bunched beams. As a result, intense beam currents can be stored and very high luminosities can be reached.

For ISABELLE, beam currents of 8 amps per ring are stacked at 29 GeV energy by multiple transfers from the AGS. These beams are then rebunched (at the third revolution harmonic), accelerated to the top energy, and debunched again. Collisions are provided in six interaction areas, four of which will be initially equipped for experiments. The two beams cross each other at 11.2 mrad in low-beta insertions with beta of 7.5 m vertically and 42.9 m horizontally. They will provide 2 x 10^{32} cm^{-2} sec^{-1} nominal luminosity with as much as 60 m free-space for the experiments. Considerably higher luminosities, at the cost of reduced free-space, can be reached by lowering the crossing angle and the beta functions in the interacting region. Maximum luminosities of about 5 x 10^{33} cm^{-2} sec^{-1} appear to be achievable in this way.

B. Comments on the ISABELLE Project

1. Management

Since N. Samios was appointed BNL Deputy Director for High Energy Physics, there has been an improvement in the ISABELLE management structure. Several critical policy decisions relative to ISABELLE have been taken (adoption of one of the new cable magnets, the 1984 goal for test of first sextant, etc.) and a reorganization is underway which should improve internal communications and more sharply delineate lines of authority and responsibility. There is a recognized need for the appointment of a Project Director and a search (difficult under the present circumstances) is underway to find a senior individual to take over the Project and accomplish the in-depth planning and scheduling, including the integration of the R&D effort into the construction program.

A planning committee has been formed and all of the magnet activities have been integrated into a revamped magnet division. Reorganization of the cryogenic, RF, and vacuum systems effort is underway and the morale of the key individuals of the Project is reasonably high, now that an attractive magnet option has been chosen. Overall morale has, of course, been eroded by the uncertainties associated with continuation of the Project, particularly in the light of recent budget cuts.

2. Magnets

BNL has decided to set aside the previous magnet design (which featured "braid" with the preload supplied by differential thermal contraction of the iron yoke) and to adopt a new design incorporating Fermilab-style Rutherford cable preloaded by bolted-up split iron yokes. As it turned out, this provided an excellent magnet which should lead to a satisfactory set of magnets for ISABELLE. The first two, short prototype magnets (CM-1, CM-2) have demonstrated excellent maximum field capability, namely 5.85 tesla (at 3.7°K) with essentially no training, and indicate that the coil and yoke design provide the necessary mechanical properties for large aperture high field magnets. The design also addresses field quality, ramp-rate sensitivity, electrical insulation, problems of differential thermal motion and quench protection.

At the time of the Subpanel's visit there was every reason to expect that 15-foot long magnets will exhibit the same stability as the 5-foot models. However, since the causes of magnet instabilities (premature quenching, etc.) are not well understood, one could not be sure of this design until a long magnet had been operated under forced-flow cooling conditions. BNL put appropriate emphasis on the achievement of this goal and a first 15-foot magnet was indeed successfully tested. It reached a maximum field of 5.35 tesla at 4.5°K with no training observed and 5.85 tesla at 3.7°K. A second full length magnet gave the same near-perfect performance. The elimination of training in such accelerator magnets is a significant achievement of the BNL group.

A preliminary plan for a magnet development program now exists which features the testing of materials and production techniques over the next several months while a production capability of one or more magnets per week and a matching magnet testing facility are developed. This program will involve the construction of approximately 35 magnets as part of the "learning curve" and lead into the production of at least

54 dipoles and 23 quadrupoles which would be installed in the existing tunnel for a sextant test. The early part of the "learning curve" phase will explore the cost effectiveness of various split yoke and support tube designs, the properties and control of epoxy used to impregnate the fiberglass insulating tape, the performance of correction coil packages incorporated into full length dipoles and quadrupoles, the ability to protect magnets with internal diodes and quench-propagating heaters, and similar items, some of which are undoubtedly not yet foreseen. All of these questions must be answered as quickly as possible in order that production of identical units may commence which will allow a test of field reproducibility and magnet reliability.

The major goals of the magnet R&D program are outlined as follows:

a. Continue to produce long coils using three iron yokes. Test six magnets by April 1982. This will establish reproducibility of training behavior and develop coil fabrication techniques.

b. By April 1982 several additional 5-foot models will have been tested, using three short iron yokes, to establish details of the end winding design for proper field quality.

c. By June 1982, one full-length dipole per week will be produced. This will continue until approximately 35 pre-production magnets have been tested.

d. By April 1984, approximately 100 magnets will have been built with 54 dipoles and 23 quadrupoles assembled into a sextant ready for test with beam.

e. By late 1984, these magnets will be cooled down and operated in the ISABELLE pulsed mode to provide an integrated systems test.

The latter portions of the production effort will provide a basis for more accurate labor and material costs for the magnets for the remaining 11 sextants.

This is an ambitious R&D schedule. With adequate financial resources and proper management, it can probably be met by the BNL group. Completion of this R&D program will provide an adequate test of magnet production capability and enable BNL to proceed with full-scale magnet production.

3. Cryogenics and Transfer Line

The overall cryogenic system for ISABELLE seems sound. There appears to be enough refrigeration capability at BNL for magnet testing. Problems experienced with prototype transfer lines, supplied by industry, seem to be due to poor design and workmanship. These can be solved by proper attention to inspection at the factory by BNL.

4. Other Accelerator Systems

The ISABELLE lattice appears to be a sound design. The arcs offer reasonable flexibility for a superconducting system, namely, the possibility of a few integers' common tune variation in the two planes and of up to one or two integers vertical-to-horizontal tune difference. The matched insertions containing the collision points offer enough flexibility to cope with unforeseen beam dynamics situations. The possibility is foreseen to lower the collision-point beta-function and the crossing angle of the two beams (with some redesign and refitting) below the nominal design values in order to gain extra luminosity at a later stage of development or to reach design luminosity should there be difficulties in achieving the design beam current of 8 amps. This seems to present a valuable safety factor in the basic ISABELLE design.

The vacuum system for a storage ring with high intensity coasting proton beams presents difficult design problems because of the need for very low pressure, efficient clearing of ionization electrons, and a high degree of cleanliness of the chamber walls to prevent pressure runaway by ion-induced desorption. The basic features of the ISABELLE vacuum system are taken over from the proven CERN-ISR design (e.g., in-situ bake-out and glow-discharge cleaning prior to installation), but additional problems arise from the need to insert the vacuum system in a superconducting magnet. The design of the ISABELLE vacuum system has reached a high degree of maturity and has undergone extensive and successful testing in the form of detailed prototypes. Production could therefore start at any time with full confidence.

The remaining accelerator systems have not received the same attention. Examples are injection, stacking, acceleration, bunch-to-bunch feedback (required to fill the ISABELLE circumference with bunches from successive AGS cycles at achievable flat-top field tolerance), beam diagnostics, and beam dumping. For most of these systems only conceptual designs or preliminary models of critical components exist, and several basic design choices have yet to be made. The acceleration system (three 3rd-harmonic cavities per ring) is considered a problem because of the combination of the required high voltage and low permitted impedance presented to the beam. Model work on this problem and on the design of the stacking cavity is in progress. For most of the other systems, detailed design work appears to be postponed to a later time in the construction schedule.

There appear to be no insurmountable problems involved in any of these systems and it does seem possible, and indeed justified under the present circumstances, to defer much of this work to a later date. It would seem urgent, however, to prepare a detailed plan and time schedule covering all the outstanding design and prototype work, from basic decisions to detailed engineering.

The stated initial luminosity goal of 2 x 10^{32} cm^{-2} sec^{-1} with 8 amps stored in each beam appears to be realistic. For comparison, the ISR with an original goal of $L = 4 \times 10^{30}$ cm^{-2} sec^{-1} at circulating currents of 20 amps per beam has now reached $L = 1.2 \times 10^{32}$ cm^{-2} sec^{-1} with the help of a low beta insertion and stored currents up to 40 amps. Thus, later achievement by ISABELLE of luminosity up to 5 x 10^{33} cm^{-2} sec^{-1} through improvements seems reasonable.

5. ISABELLE Construction Cost and Schedule

The Subpanel notes that the total of construction cost, R&D cost, pre-operating cost, and equipment cost (including detectors) from FY 1983 onwards would be about $500M (FY 1982 dollars). The Subpanel was concerned about the adequacy of the contingency allocated for magnet production in the interim cost estimate and believes that an additional $10M-15M may be required.

6. Remarks on Accelerator Physics

The ISABELLE Project will advance accelerator technology in two major ways. First, it will provide experience in this country with high luminosity proton-proton colliding beams. This experience has been acquired by CERN over the last decade, but in the United States there is no such experience. The second major contribution is experience with building and operating a superconducting accelerator. Such experience is being acquired at Fermilab, but the ISABELLE Project extends this important expertise to a second laboratory and a wider group of people.

The ISABELLE Project will also advance accelerator technology by providing experience with a different, wide-aperture, higher-field magnet than the Energy Saver magnet. The testing of a magnet string is an appropriate and important intermediate milestone. It properly focuses attention on a major integrated systems test. Coupled with a coil production R&D program this magnet R&D is essential for any hadron accelerator construction project at Brookhaven utilizing superconducting magnets.

C. Description of the TeV I Project

The TeV I project is a proton-antiproton collider which will provide a luminosity of about 10^{30} cm^{-2} sec^{-1} at an energy of 1000 GeV x 1000 GeV. To achieve this goal, protons from one booster batch are to be accelerated, in 80 bunches, in the Fermilab main ring to 120 GeV. The proton bunches are then compacted by radiofrequency manipulation from 9 nsec to less than 1 nsec, and extracted and focused to a spot of 0.38 mm on a tungsten target. The antiprotons which are created are collected by a short focal length lithium lens and are then injected into a debuncher ring. This ring, which has a central momentum of 8.9 GeV/c, has an acceptance of ±2 percent in momentum and $20\pi \times 10^{-6}$ m-rad in each transverse dimension and thus can accept 1.25 x 10^{8} antiprotons from each booster batch. The antiprotons are debunched by a factor of 10 in the debuncher ring and then extracted and injected into an accumulator ring. In this ring, by means of stochastic cooling, the antiproton density in both transverse dimensions is increased by a factor of eight, while the longitudinal (energy) density is increased by a factor of 10^4; so that finally 6 x 10^{11} antiprotons are in a core of 1.1 MeV. Approximately 6 x 10^{10} antiprotons, which are accumulated in 1.3 hours, are taken from the accumulator, and transferred to the main ring, and accelerated to 150 GeV and then transferred to the Energy Saver. This sequence is repeated three times. The resulting proton-antiproton luminosity, with a total of three bunches in each beam and a β^* value of one meter, is 10^{30} cm^{-2} sec^{-1}.

D. Comments on the TeV Project

1. Specific Remarks

The attainment by the TeV I project of luminosities of the order of 10^{30} cm^{-2} sec^{-1} usable for experiments depends on: a) the satisfactory performance of the Energy Saver ring as a storage ring; b) the success of the cooling systems in concentrating the antiproton beam to the necessary densities in phase space; and c) adequate antiproton production rates by the main ring and antiproton target system.

a) The Energy Saver ring will have a usable aperture of 15 mm over which the magnetic guide field is of sufficient quality for storage ring service. An orbit correction system is being built into the superconducting magnet ring which is designed to reduce residual orbit distortions to 1 millimeter or less. Given the expected transverse emittances at 150 GeV (the energy at which the beams are their largest) the transverse dimensions of the beams will be only a few millimeters at the worst place on the circumference. Taking orbit distortions and beam size together, only about 30 percent of the aperture need be occupied by 95 percent of the beam particles at 150 GeV and correspondingly less at 1000 GeV. These figures appear quite safe.

The designs of the low-beta insertions for the interaction regions are not yet complete. Designs for interaction regions beta-functions of 1.0 meters in both horizontal and vertical planes are in hand (at least conceptually), and these values yield $L = 0.5 \times 10^{30}$ cm^{-2} sec^{-1} with one bunch. (At the SPS an insertion with $\beta_x \leq 2$ meters and $\beta_y \leq 1$ meter is already in operation for proton-antiproton collisions.) In TeV I, luminosities up to 5 x 10^{30} cm^{-2} sec^{-1} should be attainable with multiple bunches in each beam.

The colliding beam strength parameter, ξ, (approximately equal to the beam-beam tune shift $\Delta\nu$), is only 0.002 to reach a luminosity of 10^{30} cm^{-2} sec^{-1}.

For the higher values of luminosity of 3 or 6 x 10^{30} cm^{-2} sec^{-1} the value is 0.003. For electrons where there are quantum fluctuations in the radiation and damping, there is considerable experimental information which shows successful operation of storage rings if $\xi < 0.02$. For protons there is less knowledge. The ISR has an unbunched proton beam and in normal operation no beam-beam effect has been observed for $\xi < 0.001$. TeV I has bunched beams, and hence an additional degree of freedom in the motion. First experience with proton-antiproton collisions at the SPS have shown that $\xi \leq 0.003$ is indeed an acceptable value.

b) In order to reach the proposed luminosities, the cooling ring is required to produce a cooled "core" of antiprotons in the stacked beam which is about 10^4 times denser in energy space (antiprotons per unit of energy spread) and eight times denser in each transverse phase space (horizontal and vertical) than in the injected batch. The central portion of the core must contain at least 10^{11} antiprotons per MeV within horizontal and vertical emittances of $2.5\pi \times 10^{-6}$ m-rad. (Emittances are defined to contain 95 percent of the distribution.) The concentration of the beam in the three dimensions of phase space is to be carried out exclusively by stochastic cooling systems similar, but not identical, to those used in the CERN Antiproton Accumulator now successfully in operation.

The rate of delivery of antiprotons to the cooling ring is anticipated to be about 10^8 particles every 2 seconds which leads to a collection time of 4 hours for 6 x 10^{11} antiprotons stored in the cooling ring and a central density of 10^{11} antiprotons per MeV in the core. Transfers of antiprotons to the Energy Saver for colliding beam operation will remove only 8 x 10^{10} particles for each batch transferred. At most, three such batches would be transferred to constitute a fill of the Energy Saver, and that loss could be replenished in about 1-3 hours. Four hours would be required to obtain the final goal of 5 x 10^{30} cm^{-2} sec^{-1}. If beam lifetimes of the order of a day are achieved in the Energy Saver, such collection times will not significantly degrade the average luminosity, but significantly shorter beam lifetime would threaten performance. This is an area of concern at CERN, but beam lifetimes of well over 10 hours have already been observed in the SPS proton-antiproton collider.

The CERN Antiproton Accumulator ring was designed to accumulate 6 x 10^{11} antiprotons in 25 hours. After one year of operation, it has achieved 1 x 10^{11} in about 30 hours. The TeV I design takes account of this CERN experience and overcomes the long accumulation time by means of: (i) higher production rate by means of higher energy protons; and (ii) enhanced capture via the lithium-lens, RF manipulations, and a buncher ring. Various longitudinal (radiofrequency) manipulations of the antiproton beam are required to introduce the particles into the cooling ring and to extract the central portion of the core from it. Although these procedures require precision, they are straightforward and do not appear to constitute a danger to the success of the project.

c) The antiproton production system uses a tungsten target and a lithium-lens collection system. The antiproton yields are calculated from a phenomenological fit to the world's cross-section data which shows the yield into the momentum acceptance of the cooling system to be rather flat over the range of central antiproton momenta from 8 GeV/c to 13 GeV/c. With an incident proton spot size of $\sigma = 0.38$ mm and energy of 120 GeV, the yield of 8 GeV antiprotons into the downstream acceptance ($A_x = A_y = 20\,\pi \times 10^{-6}$ m-rad and $\Delta p/p = \pm 2$ percent) is 5×10^{-5} antiprotons per proton. This spot size should permit targeting, without thermal destruction, the 2×10^{12} protons once every two seconds required to produce the 10^{8} antiprotons for the cooling system.

Fermilab already has a lithium-lens of smaller aperture while the collaborators at Novosibirsk have built another lens which would meet the specifications of the present application. There is no experience yet with a lithium-lens in a beam.

2. General Remarks

The TeV I project has certain risks associated with it such as beam manipulation by RF, stochastic cooling, beam-beam collisions with bunched non-radiating beams and efficient collection of antiprotons. On the basis of experience being acquired at CERN on the Antiproton Accumulator ring and the SPS collider, we believe that these risks are acceptable. Problems, if they arise, will often be outside the main ring enclosure and, hence, will delay colliding-beam physics, but not fixed-target physics. The attainment of a luminosity of 10^{30} cm^{-2} sec^{-1} would appear to be a reasonable goal. We note that this achievement requires efficient antiproton collection, effective cooling of the antiprotons, good manipulation and transfer of the antiprotons, and a sufficiently high beam-beam limit, but we believe that all of these requirements can be met.

The achievement of a luminosity greater than 10^{30} cm^{-2} sec^{-1} will, in our judgment, take some years of operational experience. On the other hand, a number of improvements seem possible. Thus, an ultimate goal of $L \cong 10^{31}$ cm^{-2} sec^{-1} appears reasonable to us.

A major reorganization has been accomplished by integrating the accelerator and Energy Saver effort with the TeV I effort. The Subpanel is impressed with the group and feels that it is basically quite adequate to be given the considerable responsibility of the TeV I project. However, some additional help may be required in the area of the accumulator and the damping ring.

The Subpanel notes that the total of construction cost, R&D cost, pre-operation cost, and equipment cost (including detectors) from FY 1983 onward would be about $70M (FY 1982 dollars). Detailed schedule and costs of the project are expected to be available by March 1982. The Subpanel believes that an additional $15M may be required to complete TeV I. With the annual funding constraints expected, the start up may be delayed until the end of 1985.

3. Remarks on Accelerator Physics

The TeV I project will advance accelerator technology in a number of ways. Most importantly, we shall gain experience in this country with stochastic cooling of antiprotons and with the operation of a high luminosity proton-antiproton storage ring. Experience with the operation of a superconducting ring is vitally important, but will presumably be gained with the Energy Saver project. In the future, stochastic cooling should become a readily usable tool of accelerator physics and, as such, will have many applications. Foreseeable applications are to a multi-TeV proton-antiproton project or, more immediately, to cooling in the Energy Saver ring itself. The latter could result in improved performance of the TeV I project. Thus, experience with an operational cooling system is a most valuable attribute to the TeV I project.

Future hadron colliders may have bunched beams and cooled antiproton beams, hence, the TeV I project including the associated detector developments will give experience which is applicable to future colliders.

E. Description of the Stanford Linear Collider (SLC) Project

The SLC, or Stanford Linear Collider, is a new type of colliding beam accelerator system which collides two bunches of particles accelerated in the SLAC linear accelerator at a single interaction point via a single collision, with the interaction rate made adequate by an extremely tight focus of the beams and very high charge in the individual beam bunches. The SLC provides electron-positron collisions at an ultimate energy of 50 GeV x 50 GeV and a design luminosity of 6×10^{30} cm^{-2} sec^{-1}, with a possible future increase in energy to 70 GeV x 70 GeV if that is required by the physics program. This project has a two-fold justification: the study of Z^0 physics at a center-of-mass energy of 100 GeV; and the development of a new accelerator concept which removes the energy limitations imposed on circular electron-positron colliders by synchrotron radiation.

The main components of the project are the energy upgrade of the SLAC linac; a transport system from the end of the linac to a small aperture magnet arc; the magnet arc itself; a special focusing system near the interaction point or two such systems for the alternate interaction points; the necessary housing; an experimental hall and staging area or two such areas; a high power positron-production target; a positron booster; a transport system from the positron target at the two-thirds point of the linac back to the injection end of the linac; a new high-peak-current electron gun; two small storage rings to reduce the emittances of the electron and positron beams by radiation damping; pulse compressors to reduce the length of the bunches in the storage ring before injection into the linac; and the necessary instrumentation and control systems for both the linac and the collider system.

A typical operation cycle is as follows. The electron and positron damping rings each contain two bunches of 5×10^{10} particles at an energy of 1.2 GeV. One of the positron bunches is extracted from the damping ring, passes through a pulse compressor which reduces the bunch length from the centimeter typical of the storage ring to the millimeter required for the linac, and is then injected into the linac. Both electron bunches are extracted from the electron damping ring, pass through an independent pulse compressor, and are injected into the linac behind the positron bunch. The typical spacing between bunches is about 15 meters in the linac.

The three bunches are then accelerated down the linac. At the two-thirds point, the trailing electron bunch is extracted from the linac with a pulsed magnet and is directed onto a positron-production target. The positron bunch and the leading electron bunch continue to the end of the linac, where they reach an energy of 50 GeV.

At the end of the linac, the two opposite-charge bunches are separated by a DC magnet, pass through a transport system which matches the focusing of the linac to that of the main collider ring and then begin to travel around the ring in opposite directions. The collider ring is composed of small-aperture magnets with very strong alternating-gradient focusing, which is required to hold down emittance growth in the collider arcs. After emerging from the arcs, the bunches pass through an achromatic matching and focusing section which focuses the beams to a very small size at the collision point.

The positrons produced by the "scavenger" electron bunch that was extracted at the two-thirds point of the linac pass through a focusing system at the positron source, a 200 MeV linear accelerator booster, a 180° bend and an evacuated transport pipe located in the existing linac tunnel. This brings the positron bunches back to the beginning of the linac. At this point, the positron bunch passes through another 180° bend, is boosted to an energy of 1.2 GeV in the first sector of the existing linac, and is then injected into the positron damping ring.

Because the emittance of the positron beam is very much larger than that required for collider operation, a positron bunch must remain in the damping ring for approximately four radiation damping times, which corresponds to twice the time interval between linac pulses. Thus, the positron bunch to be used in the next linac cycle is the one that is still stored in the damping ring from the previous cycle.

Electrons for collider operation are produced from a special gun equipped with a subharmonic buncher and located at the beginning of the linac. Two bunches of electrons are produced, are boosted to 200 MeV in a dedicated section of linac, and are then injected into the same section of linac used to boost the positron bunch to 1.2 GeV. At the end of this section, the 1.2 GeV electrons are injected into their own damping ring. The electron bunches at the time of injection into their damping ring have an emittance somewhat larger than required for collider operation but considerably smaller than the emittance of the positron bunch and thus need only be damped for two damping times or one interpulse period. The entire cycle repeats 180 times per second.

To achieve 50 GeV, a substantial energy upgrade is required on the existing SLAC linac in addition to the construction required to bring the two beam bunches into collision, as discussed above. Polarized electron beams are possible in the SLC.

F. Comments on the Stanford Linear Collider (SLC) Project

1. Introduction

The SLC represents a novel and exciting "window on the future" for high-energy colliding beam physics in the next decade, and by utilizing the existing SLAC linac, provides a mechanism for the U.S., to quickly achieve 100 GeV electron-positron collision energies for physics research at reasonable cost. As outlined below, the SLAC staff has addressed a host of technical questions associated with the SLC. Remarkable progress has been made in the understanding of the physics associated with such a machine, and, while the project is clearly not risk-free, it has a high probability of technical success.

Emphasis has been placed on the near-term problems associated with the conversion of the linac to collider operation. Comments follow on the individual subsystems and technical areas associated with the SLC project. The R&D associated with a very large linear collider aside from the SLC, such as very high-gradient accelerator systems or super power RF generators beyond an extension of present-day klystron technology, has not been initiated, and will not be discussed.

2. Cost and Schedule

SLAC has submitted proposals for the construction authorization of the SLC. The total cost of the project including two experimental stations (not operating simultaneously) with two sets of beam lines, detectors in both experimental halls (one of them being a major new one), a staging-area, linac improvements required for SLC, R&D, pre-operation to turn-on for physics, etc., is approximately \$170M. The cost is about \$30M less if existing detectors are used. If only one intersecting region is built, another approximately \$20M will not be required. An additional \$10-15M would be required if the klystron R&D were not successful and additional SLED II accelerating stations had to be added to achieve 50 GeV. The Subpanel recognizes that the construction of the beam transport tunnels and interaction region building are on the critical path for completion of the SLC in 1986.

3. Project Management

Because the project has not yet been officially approved or sanctioned, a project organization has not yet been established. The capabilities certainly exist within SLAC to accomplish the effort, and no major difficulties should be experienced in this regard. The part time efforts of a number of people have successfully carried the R&D and conceptual design work done so far but this will not be sufficient much longer if a FY 1986 turn-on date is to be realized.

4. Comments on the Damping Ring, 1 GeV Buncher Section, and Arc Structure

A great deal of effort has been devoted to the design of the electron and positron damping rings in order to achieve the smallest possible phase space magnitude after the short dwell time in these rings imposed by the basic 180 Hz SLC repetition rate. By proper optimization of the lattice parameters, a strong alternating gradient, small aperture, high-field structure has been arrived at which appears optimum for its intended purpose and has minimum damping time parameters. Both from the point of view of lattice structure and engineering design, the damping ring structures are not very far from state-of-the-art designs with certain original and innovative features. Magnet construction makes use of precision "stamped" laminations and infinite length busbar "coil" configurations. The vacuum system presents a problem because of the small aperture and the electron ring requires clearing of ions produced in the residual gas. The details of this clearing system remain to be designed and tested.

Because of the danger of phase space dilution by wake fields in the 1-50 GeV electron linac section, the electron beam bunch is reduced in length by a sequence of acceleration and nonisochronous magnets in the transfer section between damping ring and linear accelerator. In addition, for this transport section, special requirements exist so that beam polarization can be properly manipulated. This rather complex beam transport system has been fully designed and seems ready for construction.

For the 180° arc structures, the phase space damping associated with the RF acceleration and synchrotron radiation is absent and the optimization is guided mainly by the (undesirable) potential for beam emittance growth. This has been taken into account and a lattice structure allowing minimum emittance growth by stochastic photon emission has been designed.

Theoretical understanding exists of the phase space growth due to dynamic misalignment (typical rms limit 0.25 μm) of the arc and final focus transport elements and the actual amplitude and frequency spectrum of structure movement has been measured for the SLAC site. The results of this study indicate that uncorrectable position jitter (ν > 30 Hz) is well below tolerance level and where the jitter approaches tolerance levels, its frequency spectrum is below 30 Hz, making feedback corrections possible.

5. Comments on the SLC Positron Source

Efforts to upgrade a positron source to the required level of performance for the SLC project have already shown that a satisfactory final system can be built to yield the required positron bunch phase space density. Target materials have been tested up to the required primary electron beam density, conversion yields are known and the collection solenoid has been tested.

6. Comments on the Klystron R&D and Various Energy Options

Continuing research and development directed toward increasing the energy of the linear accelerator is carried on independently of the SLC program. That program does, however, set important intermediate goals both in time and in the required accelerator energy (50 GeV at the SLC interaction point).

The energy program currently being implemented will produce 32 GeV at the SLC interaction point. This represents the SLED I mode of operation with all present klystron stations equipped with the 36-MW klystron operating with 2.5μsec pulse width. To date about two-thirds of the stations are so equipped, and the complete conversion will require two to three more years at present rates of tube attrition and production.

SLED II requires 5μsec operation of the 36-MW klystron, and is the next step in accelerator energy upgrade regardless of the particular option chosen to reach the SLC energy specification. SLED II with the present number of klystron stations will produce 43 GeV at the interaction point. The R&D for this step has shown, however, that SLED II operation at 36 MW is marginal and that any increased energy per station beyond SLED II will almost certainly require significant changes in tube design. These changes lie in both gun and output cavity areas, with improved window performance a likely requisite also. Experimental tubes incorporating such change are being built, and two of the four options under consideration for SLC at 50 GeV involve a new klystron which is a direct descendent of the present tube along these lines. One option considers an improvement to 49 MW using the present SLED cavities; the other to only 42 MW, but replaces the SLED cavity with a more efficient design.

A third option implies the successful completion of a joint SLAC-Japanese collaboration in the development of an entirely new klystron to operate to 150 MW with a 1 microsecond pulse. This R&D program is only about six months old, and is expected to continue for about two more years. It is too early for prognosis, but if successful, this approach should be favored both by virtue of simplicity (no SLED is required) and its implications for the future.

The fourth option does not require an increased energy gain per klystron, but simply increases the number of klystron stations (operating SLED II at 36 MW) by about 30 percent. This option requires no R&D, but is the most expensive, both with respect to capital and operating cost. It is this option carried to an extreme (doubling the number of stations) and in combination with one of the other three which could increase the SLC interaction point energy to 70 GeV should that be desirable from a physics point of view in the future.

Current klystron production is sufficient for complete turnover of klystron complement in about five years. This production is limited by capital equipment rather than by personnel skills, and could be at least doubled while retaining good cost effectiveness.

7. Comments on the Beam-Beam Interaction

SLAC has studied the effect of one beam on the other extensively. The interaction will produce significant changes in beam size and scattering. Thus, the phenomenon is important in estimating luminosity and in estimating the orbits of electrons downstream of the collision point (which is important for estimating experimental backgrounds and for designing the beam dumps).

The interaction of one beam on another is measured by a dimensionless parameter, called the disruption parameter, D, which is given approximately by the ratio of bunch length to the focal length produced by the beam-beam focusing. For $D = 0.8$, the value expected for SLC, the luminosity is enhanced, due to beam-beam focusing, by a factor of 3 and this factor is included in the estimated luminosity of 6.0×10^{30} cm^{-2} sec^{-1}.

For $D=1$, the number of plasma oscillations is much less than one (~1/3) and the calculational techniques are reliable. Only for $D \sim 30$ will instabilities become important and the numerical approach need modification. The Technical Subgroup thus felt that the beam-beam effect is well-understood, as a result of the SLAC studies, and provides an adequate basis for SLC design. In particular, the luminosity enhancement, due to beam-beam effects, appears to have been evaluated reliably.

8. Comments on the Final Focus

The final focus is required to match to the beams in the arcs, and transform these beams to exceedingly small transverse cross-sections at the point of crossing. The input parameters are taken to be 5×10^{10} particles in an emittance of 3×10^{-10} rad meters (at 50 GeV), and a momentum spread of $\pm 0.5\%$.

At the present time, SLAC has three possible designs for the final focus section. One, the "microquad" solution, employs permanent (rare-earth) quadrupoles with a bore of only 1.0 mm at a distance of only 20 cm from the collision point. A second solution, the "miniquad" design also uses rare earth permanent magnets but with a bore of 1 cm. A third solution employs superconducting quads at 4.5m from the collision point and as in the "miniquad" design, chromatic aberration is corrected. The final choice has not yet been made, but the Subpanel feels that the existence of a solution has been adequately demonstrated.

9. Comments on One-Sector Results

Experiments are underway on Sector I of the SLAC linac to:

a. Develop a new high-intensity single-bunch injector;

b. Convert the present control system of the linac into a prototype for the SLC; and

c. Demonstrate that the single-bunch beam can be accelerated and injected into the electron damping ring, and generally that it can be accelerated without undue emittance growth due to wake fields.

So far, the new high-intensity, single-bunch injector has produced the required charge in a single-bunch ($> 5 \times 10^{10}$ electrons) but this bunch has not been successfully accelerated through Sector I. This is believed to be due to instrumentation difficulties. Sector I has been used to measure transverse wake field effects at lower currents by running the beam off axis in the accelerating section and observing beam spot distortion. These measurements indicate approximate agreement with the wake field theory. The emittance growth generated by transverse wake fields was considered one of the major technical unknowns in the project, and the successful measurement of this effect gives confidence that it can be successfully controlled. Sector I is expected to inject single bunches of 5×10^{10} electrons into the damping ring by the end of 1982 (when construction of the first ring will be complete).

10. Comments on the Buncher-Injector System

The R&D effort associated with the buncher-injector addresses the problem of producing a single beam bunch with the required charge and phase space specifications at the injector output (the 40 MeV point), and continues the development of high current polarized electron sources.

The buncher consists of four (two subharmonic and two fundamental) bunching sections, and has been demonstrated to capture about 80 percent of the charge emitted by a thermionic cathode pulsed for 1.5 nsec into a single S band bunch.

This part of the R&D has been successfully completed. The measurement of the output emittance represented only an upper limit and that limit was twice the SLC specification. The single bunch charge exceeded the SLC specification of 5×10^{10} electrons.

The polarized source R&D is unique in this country and we recommend its support independent of its applications to SLC. Peak current several times that required for SLC has been demonstrated. Improved cathode life and higher polarization are the goals of this program. With the nominal value of $\Delta E/E$ of ± 0.5 percent a depolarization by no more than 20 percent occurs in the arcs. On the other hand, any appreciable increase of $\Delta E/E$ will lead to a significant decrease of polarization.

11. Comments on the SLC R&D Program

SLAC has already made notable progress in their SLC R&D program. The development of a high-intensity gun and the measurement of linac jitter have gone a long way towards reducing the uncertainty--and hence risk--associated with the SLC project.

The Sector I tests show that the wake functions are not grossly different from those expected on the basis of theoretical calculations. Further studies are going on at present and will, presumably, soon give much more accurate results. On the other hand, efforts to transport a high-intensity bunch (10^{11} electrons) through Sector I were not successful. It is believed that this lack of success (i.e., emittance blow-up) is due to "trivial reasons." Further tests, with high-current bunches, are being done at present and will, hopefully, be successful. Nevertheless, the difficulty already experienced serves to show that SLC requires a very good understanding, and control, of linac performance.

It should be noted that the SLC will probably not fail--if it fails at all--in a "hard way." Rather, failure could occur partially, that is, by not achieving the design luminosity. There may be jitter in kickers so that, finally, bunches will not collide head-on, but miss somewhat; or if low-emittance bunches are not achieved then the luminosity will, again, be reduced; or if the amount of charge per bunch is limited by some unknown effect then, again, the luminosity will be affected.

The basic tests of electron bunch production, damping, and reinjection into the linac are scheduled to begin in early calendar 1983. These tests will be a very valuable indication of the future performance of the SLC.

12. Comments on the Performance Improvement and Contingency

There are a number of improvements in SLC performance which can, at least be contemplated. At this stage it is, of course, premature to seriously consider these improvements, but they do serve to show possible future directions for SLC.

Even a small increase in the number of particles per bunch, from 5×10^{10} to 6×10^{10} will increase the luminosity by a factor of 1.7. If six bunches were accelerated in the linac (rather than three) the luminosity would be increased by a factor of 2. If the crossing beta-value were reduced by a factor of 2, then the luminosity would be increased by a factor of 4. Thus, one can imagine the performance of SLC increased beyond the design luminosity of 6×10^{30} by as much as an order of magnitude at a later stage.

In case of difficulties a number of actions can be taken. For example, one can exchange bunch length with bunch transverse size and this freedom could be very useful in fighting linac tolerance. Damping rings are relatively inexpensive and one can contemplate building another one if necessary. One can also contemplate running the frequency of collisions down to 90 Hz (rather than the 180 Hz of the design). This will reduce the luminosity by a factor of 2, but it will reduce the average power requirement on klystrons, reduce the positron target requirement, while increasing damping times. Also, one can contemplate reducing the bunch size (say from 5×10^{10} electrons to 3×10^{10} electrons). The luminosity would be reduced by a factor of 4 and there would also be a reduction in the positron production requirement.

In short, there are a number of possibilities which make the gradual attainment of the design luminosity of SLC rather likely.

13. Remarks on Accelerator Physics

The SLC project will introduce several new technologies which may become part of the basis for future linear colliders of much higher energy. In essence these technologies are the acceleration of very dense bunches of electrons and positrons in linear accelerators, micro-beam focussing and beam-transport systems and the mastery of single-passage collisions.

G. Description of the CESR II Proposal

CESR II is a single-ring electron-positron collider of 5.49 km circumference, to be built at a specific site near Cornell University in Ithaca, New York. The nominal maximum energy is 50 GeV per beam, i.e., 100 GeV in the center-of-mass. The design is made for a luminosity 3×10^{31} cm^{-2} sec^{-1} at that energy. The beam optics designed to achieve this luminosity in each of the four interaction areas leaves 4 m free space for experiments in each area. The basic idea of the CESR II proposal is to provide the maximum possible luminosity at the anticipated Z^0 energy in a reasonable number of experimental areas and to do so at minimum cost. The possibility of achieving longitudinal polarization of the colliding beams without great difficulty or risk is another asset.

Large areas of the overall design are still in provisional or conceptual form only. To the extent that it is known, the design is that of a conventional electron-positron circular collider with one basically new feature: the chosen circumference is relatively small and hence the very large RF voltage of 1.5 GeV per turn is required to reach 50 GeV. To generate this voltage at reasonable power consumption and length of RF structure, superconducting cavities are compulsory. It is proposed to use a particular type of superconducting RF structure called the "muffin tin" developed at Cornell. The structure is to operate at the unusually high frequency for storage ring service of 1.5 GHz, and its total active length, made up of 1000 individual five-cell modules, is to be 500 m.

These superconducting cavities are at present still in the stage of laboratory models, not having reached nominal performance yet in any respect. Hence, most of the present effort by the Cornell group is R&D work concerned with the development of a suitable "muffin tin" structure, its testing with beam in CESR I, and preparations for an engineering design suitable for mass production. A second branch of the work is concerned with the consequences for beam stability and with other technical problems presented by the short and dense particle bunches which result from the choice of an unusually high RF frequency.

H. Comments on the CESR II Proposal

1. General Comments on Proposal-Status, Time-Scale, and Cost

Unlike the other three projects reviewed (ISABELLE, Tevatron I, and SLC), there is not yet a firm proposal for the construction of CESR II and the main emphasis of the activity is the development and testing of superconducting RF cavities. The laboratory made it clear that they would not submit a firm proposal for construction until they have satisfactorily demonstrated both that the superconducting RF cavities work and that mass production techniques for their fabrication have been established. They expect to be in a position in FY 1983 or FY 1984 to submit a proposal for an FY 1985 construction start. Adequate R&D funding for the superconducting cavity development is required ($2M in FY 1982, $4M in FY 1983, and $10M in FY 1984) in order for an FY 1985 start date to be realistic. This funding should be sufficient to demonstrate the feasibility and reproducibility of the present "muffin tin" superconducting cavity approach but is not sufficient for development of an alternative superconducting RF cavity approach. The Subpanel supports the Cornell position that additional funding for at least one such additional approach would be very desirable (for reasons explained below) to make the project ready for construction at minimal risk at the time proposed.

Although the Subpanel did not make an in-depth review of the conceptual design cost and schedule for CESR II, a 3-year construction period is considered to be optimistic. The total cost of the effort including construction, R&D, preoperating, and detectors is estimated by Cornell (on a very preliminary basis) to be about $340M (FY 1982 dollars). It is difficult to assess the adequacy of this estimate in the light of the present status of the superconducting cavity R&D. The laboratory plans to submit a more detailed cost and schedule estimate once it decides to go forward with a project proposal. Although there was some general discussion of the issue of staff required and the associated staff build-up necessary to accomplish a major project such as this, there was no discussion of project management as both the laboratory and the Subpanel thought this would be premature.

2. Expectations on Achievable Luminosity

The present overall design assumptions in CESR II, regarding two-beam behavior or beam-beam limits, are realistic and consistent with today's understanding and experience with existing electron-positron storage rings. Every attempt has been made to achieve as high a specific luminosity (luminosity per mA of beam current) as possible consistent with known limitations. Keeping the interaction regions short is an example of a lesson learned from the present generation of storage rings. However, the consequences of operating with a high betatron tune to achieve a small emittance is still under study. In all probability, this study will lead to some compromise between higher costs and attainable luminosity. Much work remains to be done on the lattice before a cost optimized design can be realized.

The area of greatest uncertainty in achieving high luminosity is in single beam limitations. The studies of transverse and longitudinal interactions between the beam and the 5000 cells of high-Q superconducting cavities, already show that to achieve longitudinal stability, the injection energy will have to be quite high. The beam behavior reacting to the transverse impedance of this structure is even more difficult to estimate. Extrapolating from instability limits in existing machines is difficult due to the very short bunch length in CESR II and the different character of the RF structure. Existing theoretical models indicate that there is a concern as to whether the required single bunch current can be achieved. This is an area which requires more theoretical and experimental work, as the leverage on achievable luminosity is large.

3. CESR I Beam Cavity Tests

A critical part of the superconducting cavity R&D program leading up to a final CESR II proposal is an active cavity test in the existing CESR I storage ring. This test represents the first experimental use of superconducting cavities in a storage ring configuration. Goals of the tests include measurements of:

- a) Effects on the beam of beam-induced modes in the cavities.
- b) Effects on the cavity of beam-induced modes.
- c) Effects due to high synchrotron oscillation frequency.
- d) Higher order mode power generation and a comparison with bench measurements of coupling impedance.
- e) Cavity survival in an accelerator environment.
- f) Beam lifetime versus voltage.
- g) Beam capture at 1500 MHz.
- h) Coupling of beam power through superconducting cavity systems.

These tests represent a critical examination of the practical problems associated with the use of superconducting cavities in storage rings and are extremely important for both a future CESR II proposal and the possible extension of technology to the next generation circular accelerator systems for high energy physics. They are receiving highest priority at Cornell, and should continue to receive this high priority whether or not the CESR II proposal goes ahead. Answers to the above listed questions are critical to determining the level of R&D support desired for superconducting cavity investigations in the future. The program seems well conceived and is being carried out in a skillful manner.

4. Comments on the Cornell R&D Program on Superconducting Cavities

The development of the CESR II superconducting accelerator structure is solely based on "muffin tin" structure made of niobium sheet and operating at 1.5 GHz.

The Cornell group has pursued the development of the "muffin tin" configuration over several years, and has progressed to the point where the mode spectrum of the cavity system is well understood and the fabrication techniques are adequate to produce cavities which have interesting Q and accelerating gradients. Grooving of the cavity surfaces seems to control multipactoring, a cause of cavity breakdown, which had limited cavity performance in the past. Study of this family of cavities through the active beam tests on CESR I is strongly encouraged, even though several new ideas have emerged during the last years which might change the final choice of system for CESR II after an adequate R&D progam. The cavity fabrication development program can produce one five-cell "muffin tin" type cavity per month in the near term, which will give a reasonable number of cavities for the CESR I tests and develop fabrication techniques at a reasonable rate. It is projected that two cavities per month can be produced in the future.

The "muffin tin" does, however, have the following disadvantages:

a) grooves covering the top and bottom surfaces are necessary to suppress multipactoring. This seems to work but the grooves increase the exposed surface and add to the manufacturing cost. By contrast, the axially-symmetric shapes now developed elsewhere are inherently safe from multipactoring as shown by computer simulation and by experiments at CERN.

b) alternate structures must be tilted by ±45° to eliminate synchro-betatron coupling and other transverse effects.

c) "muffin-tins" are not accessible to computations of fields and impedances. A circular symmetric "equivalent" is used for computations of higher modes and instabilities.

Therefore, work on a Geneva-CERN type axially symmetric cavity system should proceed to the extent possible in parallel with the "muffin tin" cavity program. The work just being started on using super-pure niobium for cavity fabrication, to achieve better heat conductivity and thus thermal stability is strongly encouraged.

The chosen frequency of 1.5 GHz is about the maximum possible for stable beam dynamics (high synchrotron tune, and short bunches). Technically, the high frequency imposes the costly 1.9°K technology and it leads to a very large number of inconveniently short modules (1000 five-cell modules, each only 50 cm long with two tuners, three waveguide ports and wasted space along the beam.) The Cornell group conceded much of this, stressing, rightly in the Subpanel's opinion, that the Cornell work is complementary to the CERN work with axially symmetric 500 MHz structures and that a final choice has yet to be made.

5. General Comments on R&D work at Cornell

The work done at Cornell in general and the R&D work on superconducting cavities in particular is of outstanding quality and represents a first rate effort in accelerator development. At present, most of this effort is directed towards CESR II and even in this specific context the field covered has been somewhat narrowed by the desire to make a specific design ready for construction in the shortest possible time, given the severe limitations of funding and manpower. The group is, however very well equipped--intellectually as well as materially--to carry out accelerator development of a wider scope, be it in the field of superconducting RF cavities for circular and linear accelerators in general or even outside that field. The Subpanel considers it of great importance that full use is being made of the outstanding potential of the Cornell Group, independent of a decision on going ahead with CESR II.

SECTION VI

NON-ACCELERATOR PHYSICS

A recent trend in particle physics is that some of the important problems are now being addressed by experiments which do not involve high energy accelerators. While experiments to search for proton decay represent the largest of these efforts, there are also such experiments, both proposed and operational, to search for quarks, monopoles, and neutrino oscillations and to investigate extensive air showers and cosmic ray neutrino interactions. We expect that the trend toward non-accelerator investigations will continue in the future.

Since some of the future non-accelerator projects are likely to be quite large and expensive, the current fraction of the high energy physics budget (1 percent) devoted to this work may need to be increased. For example, the possible establishment of a national underground laboratory for studies of proton decay and related experiments is being seriously discussed. Proposals to deploy a large neutrino facility deep underwater and to construct a large surface array for the study of primary cosmic rays are also under consideration. The construction and operation of such facilities would require significant resources, and in addition, may require an organizational structure other than the individual university approach used up to now.

We give here a brief description of current non-accelerator programs and then make a specific recommendation.

A. Neutrino Mass Measurements and Neutrino Oscillation Searches

Many experiments that focus on the possible non-zero mass of the neutrino are planned at facilities other than the major high energy accelerators. Reactors provide antineutrino beams with which one can test for oscillations via the disappearance of the $\bar{\nu}_e$'s. Medium-energy facilities, such as LAMPF, also provide low-energy ν_μ, $\bar{\nu}_\mu$, and ν_e's. Several experiments are planned or mounted which will study these neutrinos. Beta-decay studies, as in tritium, are being performed by several groups to improve present limits on the $\bar{\nu}_e$ mass and to check the recent results indicating a possible non-zero value of this mass.

B. Quark and Monopole Searches

The search for fractional charges on matter is of great consequence for understanding whether quarks can exist as isolated objects. One such experiment has reported positive results, other experiments report negative results, and there are many continuing experiments.

The production of magnetic monopoles in the early universe is expected on the basis of grand unified theories of weak, electromagnetic, and strong interactions. Designs for underground searches covering up to thousands of square meters are currently underway.

C. Cosmic Ray Physics

With the advent of large and sophisticated detectors and electronics, the time may have come to turn these techniques--so effectively applied at accelerators--to non-accelerator experiments as well. The physics of cosmic ray interactions at laboratory energies above 1000 TeV will not be studied by any other means in the near future.

The exploration of particle interactions at these ultra-high energies is currently being pursued at a number of laboratories using a variety of techniques. One example is the "Fly's Eye" detector now being brought into operation. The technique shows great promise and an extension of the present detector (costing up to approximately $20M) has been proposed. Further, a deep underground muon detector in conjunction with such an array has been proposed.

D. Deep Underwater Muon and Neutrino Detector (DUMAND)

The DUMAND (Deep Underwater Muon and Neutrino Detector) project has been proposed for studying very high energy neutrino interactions ($>$ 1 TeV) and for astrophysics studies, many of which have scientific interest whose overlap with high energy physics is growing. The prototype detector consists of an array of about 1000 phototubes deep underwater. The initial array would cost approximately $5M and take about three years to build. At present, a five phototube muon "string" is being deployed to refine the technology and to measure high energy muon fluxes. The DUMAND detector is an example of a proposed large non-accelerator endeavor which would require substantial resources and an appropriate user-oriented structure that is not at present a part of the national laboratory system.

E. Proton Decay Experiments

Among the non-accelerator experiments, the confluence of physics potential and technological feasibility is most compelling for proton decay searches. Results from the first round of dedicated experiments should be available within the next two years.

If present experiments do not reveal positive evidence for nucleon decay at the 10^{32} year lifetime level, and if the backgrounds prove to be tractable, a detector at least ten times the size of those now being used, optimized to search at the 10^{34} year level, may be deemed worth its large cost. This is estimated at approximately $40M.

If the nucleon lifetime is established at a value near the present 10^{31} year limit, a more sophisticated class of experiments would be of paramount importance. These would provide our only window to the grand unification scale of 10^{15} GeV through study of the detailed modes of nucleon decay. The ideal detector would include the capability of particle tracking, particle identification, and momentum measurement. Fine grained configurations and magnetic fields probably would be necessary. Useful measurements of branching ratios would necessitate the observation of hundreds or thousands of events. We estimate the cost of such an ultimate (> 10 kT) detector to be about $50M. It is important to encourage R&D efforts leading to an appropriate design and depth for this detector.

Despite the current preeminence of the U.S. in this field, the initiative for a large, user-oriented facility has been taken in Europe. The excavation of a cavity at a depth of 5 kmwe (kilometers water equivalent) to house a fine-grained detector with a fiducial mass of about 10 kT is already underway at the Gran Sasso laboratory near Rome. Moreover, other European groups already are proceeding with the construction of a 1.5 kT fine-grained detector at the same depth, and a Japanese group is proceeding with the construction of a 3 kT detector at greater depth than the new U.S. detectors.

F. Recommendation

We endorse the present allocation of about 1 percent of the resources of high energy physics to non-accelerator experiments. It may be desirable to increase this proportion to accommodate new facilities, and we advocate a flexible response of the funding agencies to those initiatives that are outside of the accelerator framework.

SECTION VII

FACILITY RECOMMENDATIONS AND LONG RANGE PLANS

A. Overview

The future of high energy physics promises to be as exciting and productive as its profoundly successful recent past. Whereas the U.S. program has dominated this field until recently, the importance of high energy physics has stimulated vigorous competition from other regions of the world. This Subpanel concludes that the U.S. High Energy Physics Program has the facilities that can meet the challenges of the science as well as foreign competition well into this decade, that it lays the foundations for future development beyond this decade, but that it will require the construction of a new facility available for research by the end of the decade in order for the U.S. Program to continue to be vital into the 1990's. In current plans, ISABELLE is envisaged as this facility. As discussed in Subsection C, fiscal considerations may require that a different project be constructed. The Subpanel examined a number of possible projects, but considers it premature to recommend now a specific choice.

The older facilities in the U.S. Program, including the BNL fixed-target program, the SLAC fixed-target program, SPEAR, and the 400 GeV program at Fermilab, when supported adequately, remain productive and, as history shows repeatedly, are likely to uncover major surprises in physics. Nevertheless, their roles will inevitably decline over this decade as the field advances.

CESR and PEP have already made important contributions to electron-positron physics and are expected to continue to do so over the next several years. We can expect new kinds of detectors and experiments at these facilities, but their role may be declining beyond the middle of the decade.

Tevatron II will provide a large variety of high quality beams for fixed-target experimentation beginning in the early 1980's. Slightly later, the TeV I project will have two experimental areas for studying proton-antiproton collisions at 2 TeV in the center-of-mass. The Tevatron projects will be the focus of a major part of the U.S. Program during the middle of this decade. As discussed elsewhere in this report, they will open up entirely new areas of physics and accelerator development and will be essentially unique in the world.

The ISABELLE project (as discussed extensively in the Interim Report and elsewhere in this report) is designed to provide a major base for high intensity hadron experimentation at the end of this decade and into the next.

Finally, the SLC project is expected to significantly advance accelerator R&D and provide important experimental opportunities in high energy electron-positron physics in the second half of the 1980's. However, only a relatively small number of experimental groups can be accommodated at SLC.

We conclude that the U.S. High Energy Physics Program, as outlined here, is strong and will be capable of meeting the challenges of the 1980's, but it is not without dangers. Through the middle of the 1980's the U.S. will have facilities that are competitive with those in other regions, and, with adequate utilization, will sustain an experimental program of high quality. The Tevatron I and SLC projects, along with necessary advanced accelerator R&D studies discussed below, will provide the technological basis for the U.S. to plan the major new facilities that will be required for the field to progress in the late 1990's. Such facilities may mean multi-TeV (5-20) proton rings of great size, or an electron-positron collider, probably linear, with center-of-mass energy approaching 1 TeV. New innovative technology and the physics learned in this decade may suggest other directions for the 1990's.

It is clear to us, however, that the period of the late 1980's is especially critical to the long term health of high energy physics in this country. The world-wide competition, particularly in Europe, appears to be extremely strong during this period with the start-up of LEP and possibly HERA. Furthermore, Japan will be commissioning TRISTAN, and the Soviet Union should be starting to run UNK. The U.S. Program has no provisions for any facilities that would compete directly with any of these foreign machines.

For U.S. high energy physics to remain vital beyond this decade into the era of the super-machine of the 1990's, we must maintain an active experimental program that is competitive with the rest of the world. The construction of a new facility by the end of the decade that provides major forefront experimental opportunities for U.S. physics will allow the U.S. program to remain preeminent.

The Subpanel has tried to devise the best strategy for the U.S. High Energy Physics Program. It has considered in some detail the most desirable programs which could be implemented at two budget levels, nominally \$440M and \$395M per year for DOE and correspondingly \$35M and \$34M for NSF (FY 1982 dollars). (These levels are, respectively, 4 percent above and 8 percent below the floor funding level of the 1978 DOE/OMB Agreement for high energy physics.) The program is currently operating at a level well below the \$(440,35)M figure. With respect to this we therefore interpreted it as our charge to describe a long range plan based on an approach to this level, as specified in more detail below. For the \$(395, 34)M budgetary figures our discussion assumes a steady state at that level over the decade. Finally the Subpanel has also considered the impact of a yet lower budget level of \$360M for DOE and \$32M for NSF per year.

In the discussion to follow, for the \$440M level (Subsection B) our comments focus especially on ISABELLE. This is so because, for the lower \$395M funding level, we conclude that the ISABELLE project (in its present scope) has to be abandoned. Those elements of the \$440M level program which we recommend for retention at the \$395M level will be discussed more fully in the Subsection C dealing with that level. We also discuss those new construction projects, of more limited financial scope than the current ISABELLE plan, that might be undertaken in the later half of the decade.

B. Program at the $(440, 35)M Level

1. Recommendations at the $(440, 35)M Level

At the $(440, 35)M level, the U.S. High Energy Physics Program should contain the following essential components. The order of the following list is that of our Interim Report and is not to be construed as representing priorities.

--Construction of ISABELLE at Brookhaven with completion by the end of this decade.

--Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

--Expeditious completion of the Energy Saver at Fermilab.

--Implementation of the Tevatron (TeV II) fixed-target program at Fermilab, in all three experimental areas.

--Completion of the Tevatron I (TeV I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

--Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

--Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

2. Comments on the Recommendations at the $(440, 35)M Level

These comments assume that while the immediately foreseeable budgets are likely to be below the $(440, 35)M level, this level is attained by FY 1984, or FY 1985 at the latest. The alternative situation, in which budgets remain lower, is discussed in Subsection C. We comment here mainly on ISABELLE. The remaining items are all included at the lower budget level of Subsection C, and comments on them are deferred until then.

--ISABELLE is the largest American high energy facility construction project during the 1980's, and represents a major new center for experimentation in the U.S. beginning around 1989. As a dedicated facility, it will permit a diverse broad-based research program, which can support six major experiments simultaneously. The facility is unique because of its combination of high energy and high luminosity. The high luminosity is necessary both for large statistics studies of such particles as intermediate vector bosons and for searches for rare processes and new particles. ISABELLE has a high potential for exploratory work which could lead to discoveries in the multi-hundred GeV mass range. Further, the growth potential is excellent, with electron-proton collisions and heavy ion collisions possible. It will complement both the large electron-positron and electron-proton facilities that are likely to exist in Europe at that time and also the other facilities in the U.S. Program.

The Subpanel has reviewed the ISABELLE Project in the areas of physics, technology, management, cost and schedule. As a result of the technical evaluation, it concludes that, except for superconducting magnets, the overall design is a conservative one, appropriately using many of the technical features of the highly successful CERN-ISR. The goals of ISABELLE, including the high luminosity of about 10^{33} cm^{-2} sec^{-1}, should be attainable.

With regard to the superconducting magnet design, new R&D models using the Fermilab-Rutherford cable have performed extremely well and the new design successfully addresses the key technical issues. More magnet R&D is required, however, and BNL has developed a plan to accomplish it.

Recent changes in management have been effective, but additional management changes, including the recruitment of a strong Project Director, will be required for effective program execution.

The total remaining ISABELLE cost, beginning in FY 1983 (including construction, R&D, a modest complement of detectors, and pre-operation) is estimated to be $500M (FY 1982 dollars). The average yearly support levels of $440M DOE and $35M NSF include the ISABELLE costs and a lean level of support for the remainder of the program. Since we are describing a plan based on an approach to the above funding level, we recognize that some retrenchment of the ISABELLE activity is required for the near future. For that interim period we, therefore, recommend a concentration devoted chiefly to R&D on the basic magnet, magnet production development, and magnet string tests.

It must be emphasized that at this overall budget level the funds available for ISABELLE detectors are quite modest. Furthermore, this plan does not include other desirable facilities such as a major electron-positron facility or a major electron-proton collider.

In summary, the Subpanel reaffirms the recommendation of the 1977 and 1980 HEPAP Subpanels and strongly recommends that the ISABELLE Project be built and completed during this decade. This investment in the program will enable the U.S. to maintain an active and healthy high energy physics program near the end of the decade and into the 1990's.

C. Program at the $(395, 34)M Level

This Subsection deals with the situation in which funding over the decade is assumed to be constrained to the $(395, 34)M level. The primary distinction between this and the higher level discussed above is the ISABELLE project for a high luminosity, high energy proton-proton collider. Our conclusion for the lower funding level is that construction of this scientifically valuable facility would have to be abandoned in its present scope. Between a steady $(395, 34)M level and the higher level, which assumes an early approach to $(440, 35)M, there is a difference of opinion within the Subpanel as to where the line should be drawn for abandonment of the ISABELLE project. Indeed, some members felt that inclusion of ISABELLE construction even at the higher $(440, 35)M level would jeopardize funding for the other elements of the national program. Others felt that the Project must be sustained even at the $(395, 34)M funding level. The majority was of the opinion that it should be included at the higher level and not at the lower. The Subpanel strongly believes that a new facility, more modest in cost, should be completed by the end of the decade.

1. Recommendations at the $(395, 34)M Level

--Adequate utilization and maintenance of existing accelerator and storage ring facilities, and support of important non-accelerator particle physics projects.

--Expeditious completion of the Energy Saver at Fermilab.

--Implementation of the Tevatron II (TeV II) fixed-target program at Fermilab, in all three experimental areas.

--Completion of the Tevatron I (TeV I) proton-antiproton collider at Fermilab, including the provision of a major detector facility.

--Continuation of the major R&D effort at SLAC on the Stanford Linear Collider (SLC), with construction of associated conventional facilities to begin in FY 1984 such that the R&D phase can be completed in FY 1986. Funding of major new detectors and provision of a second beam-beam intersection region could occur once the R&D program has demonstrated adequacy for expanded physics research.

--Pursuit of other advanced accelerator R&D activities on items such as high field superconducting magnets, high gradient accelerating RF structures, superconducting RF cavities, and novel means of acceleration.

--With the present ISABELLE project terminated, a start by the mid-1980's on a new high energy construction project of more modest financial scope. Although it is premature to recommend a specific direction at present, the Subpanel is aware of a number of interesting concepts currently being considered by the community and urges intensified discussion, development, and R&D aimed at selection and authorization by the middle of the decade.

2. Comments on the Recommendations at the $(395, 34)M Level

--Utilization of present facilities. At present the U.S. high energy physics experimental effort is accomplished at Fermilab with its 400 GeV proton facilities, at SLAC on the 15 GeV electron-positron PEP facility, the 3.5 GeV electron-positron SPEAR facility and the 25 GeV electron fixed-target facility, at Cornell on its 8 GeV electron-positron CESR facility, and at BNL with its 30 GeV proton fixed-target facility. Utilization of these accelerators and storage ring facilities provides the lifeblood of the university program in high energy physics, and maintains the flow of outstanding young scientists into the field. This flow is essential to the continued preeminence of the U.S. High Energy Physics Program. In the last several years relatively low operating funding, combined with the R&D needs for approved construction projects, has led to an inadequate utilization level of existing facilities. For example, both at Fermilab and Brookhaven, the number of accelerated protons has dropped to about half the maximum levels reached in 1979. Important experiments are not performed, are deferred or stretched-out unreasonably, or are terminated prematurely. The Subpanel recommends more support for accelerator operations and the physicists who use these accelerators. Every increase in support has an extremely high leverage in terms of increased operations. In the latter half of this decade when the new facilities begin operations, the program on older facilities such as the AGS and the SLAC fixed-target will be decreased or terminated in order to fund operation of the new facilities.

As discussed in more detail in Section VI, a modest fraction of high energy funds should go into the support of important non-accelerator particle physics projects.

--The expeditious completion of the Fermilab Energy Saver together with utilization of existing facilities, remains the highest priority of the U.S. Program. Its construction is well underway and operational trials are scheduled for 1983. Not only will this device be the world's first superconducting synchrotron, it is also the prerequisite facility for the completion of TeV I and TeV II discussed below. Together, these Tevatron facilities will become the backbone of the university user program. This new accelerator, with the world's highest energy throughout the 1980's, is also a major advance in accelerator technology. The successful operation of the Energy Saver will give confidence that future superconducting-magnet accelerators and storage rings can be built. Moreover, the Energy Saver will provide enough economy in electric power consumption to permit year-round operation of the accelerator.

--TeV II, with secondary beams of energies up to 1 TeV, will be the world's major center for fixed-target experimentation through the 1980's. It will open a new energy region, and provide broad opportunities for many kinds of experiments. These include studies of the quark structure of a variety of hadrons, searches for the tau neutrino and neutrino oscillations, studies of hadrons containing heavy quarks, and investigation of a variety of rare phenomena including CP violation.

--The TeV I proton-antiproton collider with 2 TeV available total energy represents an exciting step of relatively modest cost to a new frontier in hadron-hadron physics. Throughout the decade and into the 1990's, TeV I will attain the highest center-of-mass collision energy of any facility in the world. Important experiments will include studies of both the charged and neutral intermediate bosons W and Z^0, searches for heavier such particles, and searches for composite structure of the basic constituents of matter. This project will advance accelerator technology in a number of ways and provide invaluable experience in the cooling of high energy particle beams.

--SLC construction serves two purposes. First, SLC represents a dramatic advance in the field of particle accelerators, which provides the means for a thorough investigation and demonstration of the technology of the collision of micron-size electron and positron beams. Such an investigation is a crucial step in the design of very high energy linear colliders for the next decade. The second purpose is to carry out the early exploration and systematic study of the very rich and important Z^0 decay modes and of the associated electroweak physics. The Subpanel feels that it is important that these purposes be achieved as early as possible, and therefore, recommends construction authorization in FY 1984.

The Subpanel recognizes that SLC requires the use of novel accelerator technology and the adaptation of a linear accelerator not built for that purpose. Therefore, this construction authorization (about $80M) should be limited to the conventional facilities, beam manipulating systems, beam transport systems, positron source and other items required to obtain beam-beam interactions in the 100 GeV range at one interaction point. This will allow comprehensive studies to be made of the performance characteristics of this exciting new accelerator technology, and the luminosity is expected to be sufficient for early studies of Z^0 physics. The total cost for SLC R&D, including construction work and associated operating activity is $120M (FY 1982 dollars) beginning in FY 1983, with construction completion in 1986, assuming authorization in FY 1984.

The expansion of the physics capability of SLC with a second interaction region (with its associated beam transport systems) and extensive new detector construction will require an additional expenditure of about $50M. The authorization of that expenditure should be paced by the successful progress of the R&D work under way. The major measure of that progress is that on the basis of this R&D a luminosity of at least 10^{29} cm^{-2} sec^{-1} can be reached with reasonable confidence. This will allow the study of 50,000 Z^0 decays per year. (The luminosity expectation of 6 x 10^{30} is expected to yield 3 million Z^0 decays per year.)

--Advanced accelerator R&D must remain an essential component of the U.S. Program to provide the tools and techniques which will make the accelerators of the future technically and economically feasible.

In particular, the Subpanel endorses the recommendation made by the "Tigner Panel" ("Report of the Subpanel on Accelerator Research and Development of the High Energy Physics Advisory Panel," June 1980) that advanced accelerator R&D should be funded at a level of effort up to about 4 percent of the U.S. High Energy Physics Program, which is a significant increase over the present level of effort.

As already pointed out, both the TeV I and the SLC projects have strong R&D components which indeed provide part of the justification of these efforts. There are, however, other R&D areas which must be urgently pursued. The development work on superconducting RF cavities at Cornell should be continued and expanded in scope. This outstanding work opens the possibility of constructing a more cost effective future electron-positron facility. Other R&D areas with important potential payoff for the future include high field superconducting magnets, high gradient accelerating structures, cooling techniques, and novel methods of acceleration.

There is the need to hold workshops on special topics in accelerator physics, and also to teach and train graduate students, postdocs, and young scientists. Most importantly, there is the need to examine accelerator systems which are not constrained by location, size, geology, and existing facilities of the present centers.

--A new accelerator project, whose design and construction should be initiated by the mid-1980's based on earlier development work, is a crucial aspect of this program. Without ISABELLE, the completion of a new accelerator capability by the end of the decade is important for several reasons:

a) By 1990 most of the older accelerators will have been in use for at least a decade, the range of new physics they can illuminate may be shrinking, and there will be important scientific questions that cannot be addressed by these machines.

b) At the TeV I and SLC facilities, the number of available interaction regions will be limited.

c) Opportunities for adequate University involvement in high energy physics will need expansion.

A number of new and exciting facility ideas are now being studied by the community, and still other schemes are likely to emerge in the near future. We believe that the time will be ripe for selection and authorization of a construction project by the mid-1980's. In preparation for this, we urge that intensified discussion, development, and R&D begin as soon as possible.

These ideas are still in preliminary form for the most part, and the Subpanel has therefore not attempted to make choices among them. Some examples of new projects of appropriate scale under current study in the community include: an electron-proton collider or a less expensive high luminosity ($L \sim 10^{33}$ cm^{-2} sec^{-1}) hadron-hadron collider built in the ISABELLE tunnel; a second proton collider ring at Fermilab dedicated to proton-antiproton, proton-proton, and/or electron-proton collisions; an electron-positron collider using superconducting RF cavities (as proposed for CESR II); or a combination of smaller facilities, one of which might be a major non-accelerator facility. Many of these projects are represented by a considerable R&D effort at present (for example, that now devoted to ISABELLE at Brookhaven or CESR II at Cornell). It is important that these efforts be encouraged to continue, and, in some cases, be increased and diversified.

It appears likely that the cost of such a project may be in the range of $250-$300M. If the cost were higher, its completion would be delayed well into the 1990's with two disadvantages: (i) corresponding delay in its exploitation at a time when other facilities were being retired, and (ii) possible delay in the initiation of a large new ultra-high energy facility in the beginning of the 1990's.

3. Impact of the $(395, 34)M Level

In conclusion, the program at the $(395, 34)M level is a substantial one. The opportunities for experiments provided by the presently existing facilities and those which will be completed in the next several years will maintain the momentum in the U.S. High Energy Physics Program until late in the 1980's. Near the end of the decade, however, the absence of a major new facility such as ISABELLE will be apparent. A modest new facility will help to fill the gap, but will necessarily provide more modest opportunities than would ISABELLE, and would probably turn on later. In the next decade, the U.S. effort, while of high quality, would be less competitive.

D. Impact of Funding the High Energy Physics Program at $(360, 32)M Level

To be responsive to the Charge, the Subpanel presents here a brief impact report on a yet lower level, equivalent to a $(360, 32)M level of funding. As outlined above, a reduction from $440M to $395M in DOE funding would require the termination of the ISABELLE project so as to preserve reasonable levels of utilization of existing facilities and to insure some flexibility in research and development work leading to vital future facilities. An additional reduction to a spending level of $360M per year would lead to further attrition of physics output, personnel, and the ability to carry out competitive research in future years.

Such a level of funding continued through this decade would have the following impact:

1. Termination of the ISABELLE project.

2. Impossibility of a new initiative in the mid- to late-1980's as included in the $395M budget.

3. One or more of the following actions: early termination of the AGS and SLAC fixed-target programs; abortion of the SLC project; termination of the TeV I collider project; sharp reductions in utilization at all laboratories.

These actions would have very serious negative implications for the maintenance of a successful high energy physics program. Termination of both ISABELLE and the AGS would leave Brookhaven without any high energy physics facility. Failure to construct the SLC would eventually have a similar impact on SLAC. Moreover, it would close off one of the important avenues to very high energy physics by preventing essential research for the linear collider concept. Termination of the TeV I project after the elimination of ISABELLE would mean the total abandonment of very high energy hadron physics in this country. Finally, any further reductions of utilization would bring high energy experimental physics to a near stand-still. Many physicists would undoubtedly leave the field and this irretrievable loss of competent and highly trained personnel would perhaps be the most damaging result. The result of the actions outlined above would represent a staggering blow to the health of the U.S. High Energy Physics Program. The program would be headed for a serious decline. It is important to emphasize that the present ongoing program is living on the construction investments of the past. If such investments are sharply curtailed in the present, the future will be bleak. Once lost, the preeminent U.S. position could be restored, if at all, only after many years of improved support.

SECTION VIII

INTERNATIONAL COLLABORATION

Science has always benefitted from the widest possible exchange of ideas, information, and experience and this is particularly true in the field of high energy physics. International exchanges occur through conferences, publications, and visits but are even more effectively realized when physicists work in the institutions of other regions. The exchange of physicists and groups can allow a more diversified program of physics in each region, unless the complete range of accelerator facilities can be provided on a national or regional basis. As the cost of high energy machines increases, the feasibility of such provision for each country or region diminishes and the international use of those facilities that can be provided become highly desirable. The alternative would be a narrow physics specialization in each country which would inevitably stunt the advancement of the field. As long as such exchanges go in both directions, they represent little real increase in cost to any one country, and the benefits are shared by all. They should clearly be encouraged.

Cooperation across national boundaries has so far taken a wide spectrum of forms:

a. Individuals from one country working at laboratories in other countries for periods of a year or more, supported either by their home institution or by the host institution. This liaison has been especially strong between the U.S. and Europe, in both directions, but also exists to a lesser degree with Japan and the USSR.

b. Groups from one country performing experiments at a facility in another country, with or without local collaborators. Some examples within the U.S. program are the American groups at the CERN-ISR, and at PETRA, and the various European university groups at Fermilab, SLAC, and Cornell. Such collaborations, motivated by the physics interests of the scientists involved, have formed in much the same way as those which do not cross national boundaries. Program committees at the major U.S. and European laboratories have generally approved experimental proposals on their scientific merit without regard to the national affiliations of the proposers. In January 1980 a working group of U.S. and European physicists (Mulvey, Peoples, Strauch, and Weber) surveyed the record of such collaborations over the 1974 - 1980 period and concluded that "The collaborations between the U.S. and Western Europe have made significant contributions to the success of the programs in each region. There was a very good balance in the efforts in each direction during the period investigated."

c. Collaborations involving major transfers of equipment or funds between countries. These are usually negotiated between the responsible Government agencies. Examples include the Japanese participation at Fermilab, SLAC, and BNL, the Soviet participation at Fermilab, and the French participation at Serpukhov.

d. Collaborative accelerator construction projects. The most conspicuous example of successful international cooperation in accelerator construction is the series of large facilities including the PS, the ISR and the SPS built at CERN through the joint effort of 12 European countries.

Because of the enormous costs of the ultra-high energy facilities which may be needed in the future, the notion of collaborative accelerator construction projects will probably have to be extended to a world-wide collaboration to build machines beyond the means of any one country or region. The International Committee for Future Accelerators (ICFA) with representatives from the USSR, Western Europe, Japan, and the U.S. has been the group in which this issue has been considered and discussed. In its November 1981 meeting, ICFA recommended the setting up of two task forces, one to study and design a 20 TeV proton accelerator and the other to study and design an electron-positron collider of at least 300 GeV energy per beam. ICFA's stated aim is to start construction of one or both machines in about 10 years.

The Subpanel has considered the whole area of international collaboration including the ICFA recommendations and submits the following recommendations.

A. Recommendations

1. Laboratories and funding agencies should help support participation of individuals and groups in experiments at facilities not available in their own countries.

2. The U.S. funding agencies should help to support outstanding joint ventures at CERN, DESY, or other non-U.S. high energy laboratories to provide capabilities not available in the U.S. In the other direction, U.S. laboratories should also be receptive to such projects.

3. The U.S. should entertain the possibility of major bilateral accelerator construction projects with Europe or Japan, if they are cost effective and have strong physics motivations.

4. The Subpanel supports collaboration with the ICFA initiative in laying the groundwork for the large preparatory effort required before a world-wide super-accelerator can be built. However, the Subpanel does not recommend the commitment of significant U.S. resources to the ICFA project at the present stage of international collaboration.

B. Comments on the Recommendations

1. This form of international cooperation has been active for some time. SLAC, DESY, and Cornell have long had foreign representatives as members of their program committees, and a similar policy might also be considered at Fermilab, CERN, and Brookhaven.

2. Examples of the joint ventures under consideration would include the construction of major detectors, or the equipping of new experimental areas.

3. In the construction of accelerator facilities beyond the means of any one country or region, bilateral arrangements might be a natural step between the present regional facilities and a world-wide collaborative construction effort.

4. In the further future, perhaps in the 1990's, a world-wide super-accelerator project of the sort advocated by ICFA may become attractive. Such a project would require many years of administrative and scientific preparatory work. The Subpanel supports collaboration with the ICFA initiative to assist in laying this groundwork. Such collaboration may also stimulate new initiatives in accelerator R&D. However, until international collaboration has advanced considerably along the lines mentioned above, the Subpanel does not believe it is appropriate to commit significant U.S. resources to the ICFA projet.

In the context of Recommendations 1 and 2, examples of specific accelerator facilities which will be unique and therefore especially appropriate for world-wide use include the following:

a. Fermilab Tevatron II complex which will, for some time, provide the highest energy fixed-target collisions. It can and does provide good opportunities for non-U.S. users.

b. The ISABELLE facility which, if constructed, will be unique in the world and could provide good opportunities for foreign participation, particularly in view of the two experimental areas that are not initially to be developed.

c. The LEP facility which will produce the highest electron-positron collisions. The four initially undeveloped areas will likely attract U.S. experimental groups.

d. The HERA facility which, if built, would offer the highest enegy lepton-hadron collisions. This project is now actively seeking involvement by other countries.

e. The TRISTAN electron-positron (and eventually electron-proton) collider facility in Japan, which may also be appropriate for participation by outside experimental groups.

SECTION IX

LONG-RANGE PLANNING

Long range planning for the U.S. High Energy Physics Program involves the complicated interplay of many different groups including the funding agencies, the accelerator laboratories, the community of experimental and theoretical high energy physicists, and advisory bodies such as HEPAP and Subpanels to HEPAP. The most visible part of this planning process in recent years has been the series of HEPAP Subpanels arranged on an ad hoc basis to make recommendations on construction of new accelerator and storage ring facilities and to consider program priorities under specific budget assumptions. The present Subpanel is the fifth in the recent series that began with the Weisskopf Subpanel in 1974.

It is evident to this Subpanel that there are a variety of concerns in the high energy physics community about present methods of long range planning. For example, there is some feeling that the high energy community does not have adequate opportunity to participate in this planning process which so affects its future. Another difficulty is the short time scale available for reaching decisions on complicated issues. Finally, there seems to be a clear need to inform better the entire community about the issues related to long range planning and facility construction.

The above list of concerns is certainly not complete but it does point out the need for improvement in the procedures for long range planning. It is essential, of course, to stress that no committee or other planning structure, however wise its membership, can be a substitute for the creativity that must come from individuals within the field. Such committees can react to proposals, can help create the climate in which scientists can get together and generate new ideas, and can attempt to rank the relative merits of competing proposals. They cannot by themselves generate new projects. New initiatives must come from the people who are going to do the work. It is natural and commendable that the national laboratories with large accelerator expertise are the places where much of the creative work in the development of new facilities is done.

For purposes of short-term planning, the HEPAP system seems to have worked well. Its reputation in the Government is good; outstanding scientists have served on it, and it has been led by distinguished Chairmen.

The Subpanel has discussed at some length possible ways to improve the process of long range planning. One possible planning mechanism seriously considered was the formation of a joint HEPAP-DPF standing committee on long range planning whose functions might include: (1) the sponsorship of DPF workshops on new facilities or on the exploration of new technologies relevant to detectors or accelerators; (2) the issuance of occasional reports on the state of high energy physics and future planning; and

(3) the preparation of specific reports for HEPAP and the funding agencies as requested. Thus, part of the function of such a committee, perhaps augmented by some ad hoc members, might be to carry on the studies presently done by the ad hoc Subpanels on facilities and on long range planning. The advantages of having such a group would include the greater DPF involvement and interaction, the greater continuity of the planning activity, and the broadening of the issues considered.

In spite of these good features, the Subpanel could not convince itself that these advantages over the existing systems were sufficiently compelling to justify recommending at this time that a new formal structure be set up. HEPAP and the DPF Executive Committee may wish to consider in more depth than has been possible for the Subpanel whether such a standing committee might be an appropriate substitute or addition to the present ad hoc groups. Aside from this structural question, it would seem appropriate to the Subpanel if DPF on its own made a larger effort in the planning area. This effort might involve such activities as sponsorship of appropriate workshops as already discussed and increased scheduling at its meetings of invited talks in areas relevant to future planning. Indeed DPF may wish to set up on its own (without HEPAP or funding agency involvement) a small committee specifically charged with the implementation of such planning activities. The existence of such a group, if effective, might lead in an adiabatic way to a full-fledged committee on long range planning such as that mentioned earlier.

APPENDIX A

CHARGE
SUBPANEL ON LONG RANGE PLANNING FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM

Within the context of changing worldwide high energy physics activities and opportunities, review the status and prospects of the U.S. program (DOE & NSF) taking into consideration all aspects of the program.

Develop a strategy and long-range plan for the U.S. Program over the next decade under the following funding constraints:

1. The DOE/OMB Agreement of 1978 which is equivalent to a DOE level of $423M in FY 1982 dollars. The NSF FY 1979 level of $25M for elementary particle physics is equivalent to $35M in FY 1982 dollars.
2. A level of 10 percent lower.
3. A level of 10 percent higher.

Within each of the above scenarios assess the prospects and sufficiency of the strategy and long-range plan to maintain a forefront U.S. High Energy Physics Program.

In view of the pivotal role which the ISABELLE project has on long range planning please submit an interim report which evaluates the priority of ISABELLE in each of the three constraint cases, taking into consideration the physics potential of the facility, the funding and schedule required to complete construction, and competing needs and opportunities.

The interim report should be submitted by November 1, 1981, and the report should be completed by January 1, 1982.

APPENDIX B

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Subpanel on Long Range Planning
for the U.S. High Energy Physics Program

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